



This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

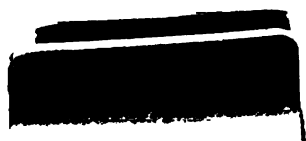
Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

- + *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.
- + *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.
- + *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.
- + *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

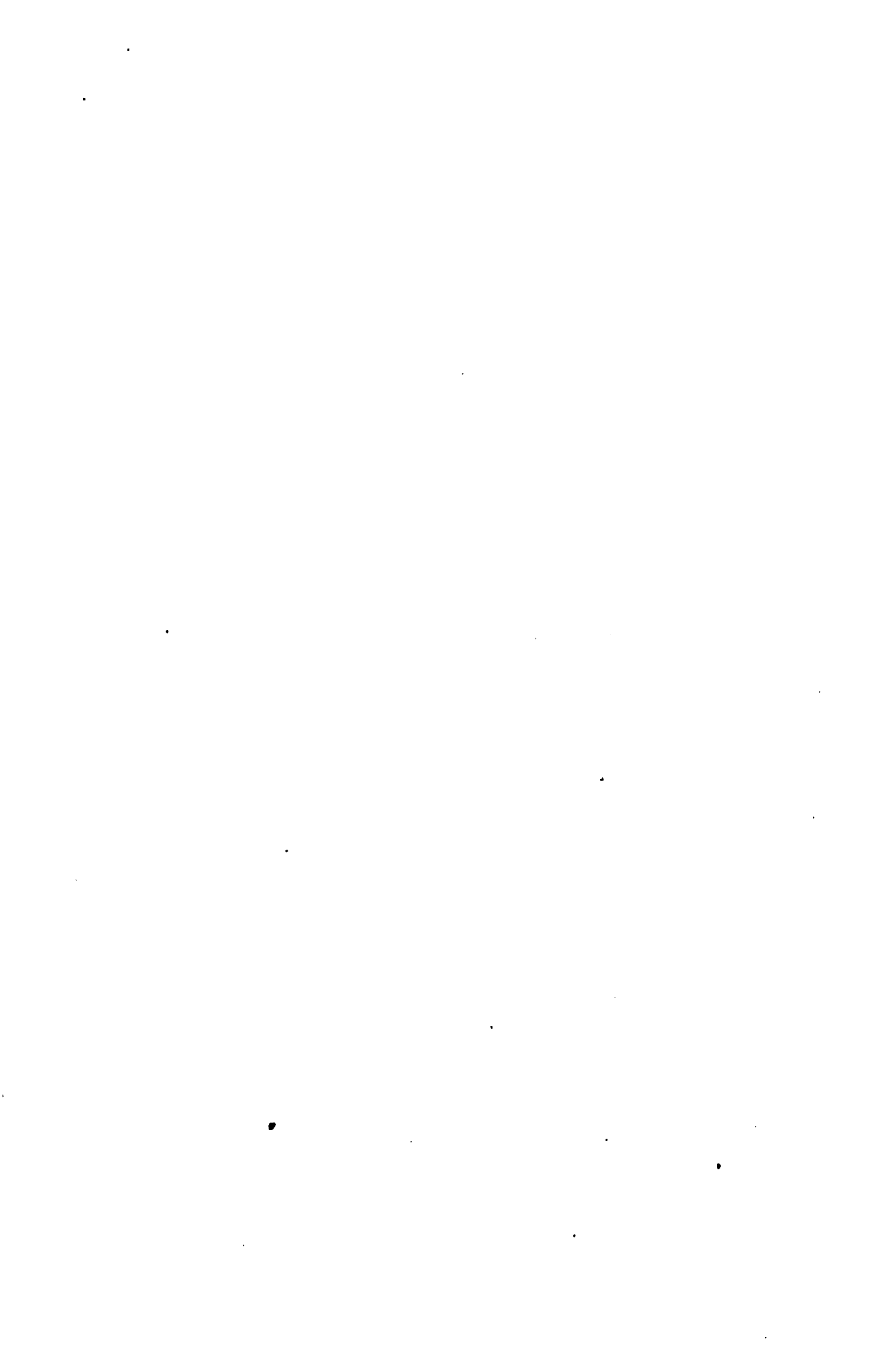
About Google Book Search

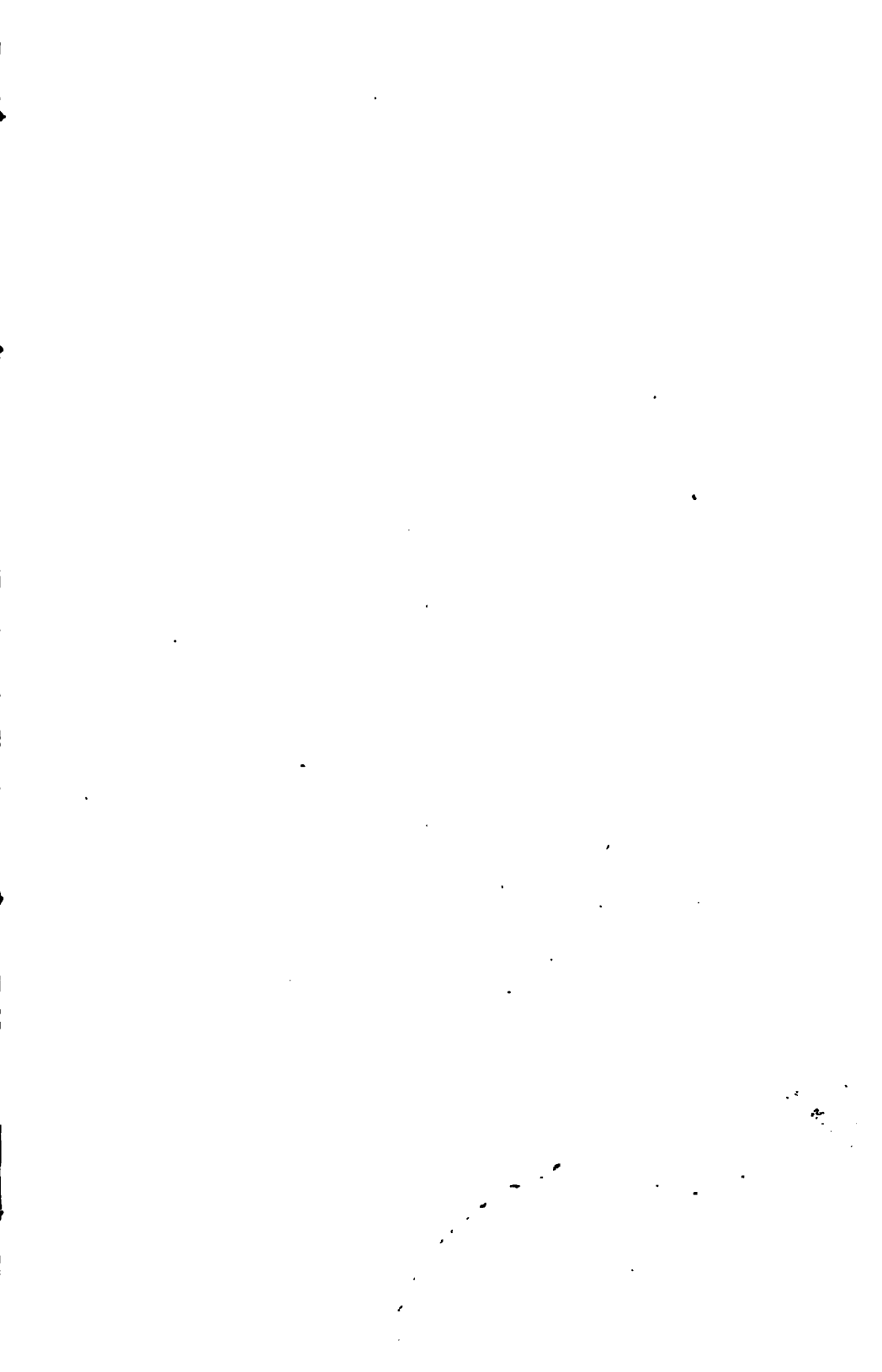
Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at <http://books.google.com/>



SCIENTIFIC & TECHNICAL PAPERS
OF
WERNER VON SIEMENS.

VOL. I.







J. W. Siemens

SCIENTIFIC & TECHNICAL PAPERS

OF

WERNER VON SIEMENS.

*TRANSLATED FROM
THE SECOND GERMAN EDITION.*

VOL. I.

SCIENTIFIC PAPERS AND ADDRESSES.

WITH ILLUSTRATIONS AND A PORTRAIT OF THE AUTHOR.

LONDON :
JOHN MURRAY, ALBEMARLE STREET.

1892.

[All Rights reserved.]

LONDON :
BRADBURY, AGNEW, & CO. LD., PRINTERS, WHITEFRIARS.

TK7
S5
v. 1

PREFACE.

THE first German edition of Dr. Werner von Siemens' papers and addresses has been received with so much interest in extensive circles, on account of their scientific and technical importance, that the demand has arisen for a second edition, which the publishers have had much satisfaction in supplying; this edition will consist of three volumes, and is intended to afford an exhaustive insight into the whole of the scientific and practical career of a contemporary prominent alike as a scientific man and as a manufacturer.

The present volume is a translation of the first volume of the second German edition, and contains the scientific papers and addresses of the first edition, with the addition of some older and some more recent papers.

This collection is now published for the first time in this country and will it is believed prove Dr. Werner von Siemens to be one of the foremost among the many workers who during the present century have revolutionized the manner of living by developing science and by applying its methods to the problems of every day life.

Among the more interesting papers of this volume are those on Duplex Telegraphy, Electrostatic Induction, the Mercury Unit of electrical resistance, laying and testing Submarine Cables, the dynamo-electric principle, the in-

fluence of light on the electric resistance of selenium, and many others the subjects of which deal with nearly every branch of science.

A translation of the second volume of the German edition comprising technical and other papers on domestic and industrial progress at home and abroad is nearly ready. These essays, although written for special occasions, can claim by common consent to be of permanent value.

In an appendix to the second volume will be found descriptions of apparatus, instruments, and inventions which originated with the Author, and have been of importance to technical progress.

A third volume will shortly appear in Germany giving an account of the remarkable career of Dr. Werner von Siemens, and in connection with it of the progress of the renowned firm of Siemens and Halske, and of the part it has played in raising electro-technology to its present importance.

CONTENTS OF VOLUME I.

	PAGE
ON THE EMPLOYMENT OF HEATED AIR AS A MOTIVE POWER	1
APPLICATION OF THE ELECTRIC SPARK TO THE MEASUREMENT OF VELOCITY	7
REMARKS ON THE ELECTROLYTIC DEPOSITION OF NICKEL AND COBALT	14
ON TELEGRAPH LINES AND APPARATUS	15
MEMOIR ON THE ELECTRIC TELEGRAPH	29
ON FORWARDING SIMULTANEOUS MESSAGES THROUGH A TELE- GRAPH CONDUCTOR	64
ANSWER TO EDLUND'S REMARKS ON THE SENDING OF SIMULTA- NEOUS MESSAGES	80
CORRECTION OF THE FINAL REPLY OF MR. EDLUND ON THE DUPLIX TELEGRAPH	84
ON ELECTROSTATIC INDUCTION AND RETARDATION OF THE CURRENT IN CORES	87
REMARKS ON WHEATSTONE'S AUTOMATIC WRITING TELEGRAPH	136
OUTLINE OF THE PRINCIPLES AND PRACTICE INVOLVED IN TEST- ING THE ELECTRICAL CONDITION OF SUBMARINE TELEGRAPH CABLES	137
DESCRIPTION OF UNUSUALLY STRONG ELECTRICAL PHENOMENA ON THE CHEOPS PYRAMID NEAR CAIRO DURING THE BLOW- ING OF THE CHAMBERS	159
PROPOSAL FOR A REPRODUCIBLE UNIT OF ELECTRICAL RESIST- ANCE	162
ON UNITS OF RESISTANCE AND THE DEPENDENCE OF THE RESISTANCE OF METALS ON HEAT	180
STANDARDS OF RESISTANCE	192
ON THE HEATING OF THE GLASS WALL OF THE LEYDEN JAR BY THE CHARGE	192
ON THE UNIT OF ELECTRICAL RESISTANCE	194
ON THE LAW OF THE MOTION OF GASES IN TUBES; ON THE PNEUMATIC DESPATCH OF MESSAGES IN BERLIN	207
METHOD FOR CONTINUOUS OBSERVATIONS OF THE TEMPERATURE OF THE SEA WHEN SOUNDING	215
OF THE CONVERSION OF MECHANICAL ENERGY INTO ELECTRIC CURRENT WITHOUT PERMANENT MAGNETS	217
DEAD-BEAT NON-ASTATIC MAGNETS	220

	PAGE
CAPILLARY GALVANOSCOPE FOR THE MEASUREMENT OF RESISTANCE IN SUBMARINE CABLES	222
DIRECT MEASUREMENT OF THE RESISTANCE OF GALVANIC BATTERIES	224
INAUGURAL SPEECH OF DR. SIEMENS AND REPLY OF PROF. DU BOIS REYMOND, SECRETARY OF THE PHYSICO-MATHEMATICAL SECTION	231
CONTRIBUTIONS TO THE THEORY OF LAYING AND TESTING SUBMARINE TELEGRAPH CABLES	237
ON THE INFLUENCE OF LIGHT ON THE CONDUCTIVITY OF CRYSTALLINE SELENIUM	263
MEASUREMENT OF THE VELOCITY OF TRANSMISSION OF ELECTRICITY IN SUSPENDED WIRES	264
ON THE DEPENDENCE OF THE ELECTRIC CONDUCTIVITY OF SELENIUM ON LIGHT AND HEAT (PART I.)	275
ON THE DEPENDENCE OF THE ELECTRIC CONDUCTIVITY OF SELENIUM ON LIGHT AND HEAT (PART II.)	296
ON THE ELECTROMOTIVE ACTION OF ILLUMINATED SELENIUM DISCOVERED BY MR. FRITTS OF NEW YORK	318
PHYSICAL AND MECHANICAL CONSIDERATIONS SUGGESTED BY THE OBSERVATION OF AN ERUPTION OF VESUVIUS IN MAY, 1878	321
ON THE DEPENDENCE OF THE ELECTRIC CONDUCTIVITY OF CARBON ON TEMPERATURE	342
CONTRIBUTION TO THE THEORY OF ELECTRO-MAGNETISM	353
ON THE LUMINOSITY OF FLAME	372
ON THE ADMISSIBILITY OF THE ASSUMPTION OF AN ELECTRICAL SOLAR POTENTIAL, AND ITS IMPORTANCE FOR THE EXPLANATION OF TERRESTRIAL PHENOMENA	377
A CONTRIBUTION TO THE THEORY OF MAGNETISM	400
ON A CONTRIVANCE FOR REPRODUCING THE UNIT OF LIGHT PROPOSED BY THE PARIS CONFERENCE FOR THE DETERMINATION OF ELECTRIC UNITS	418
ON THE UNITS OF ELECTRICITY AND LIGHT ACCORDING TO THE RESOLUTIONS OF THE PARIS INTERNATIONAL CONFERENCE	419
ON THE CONSERVATION OF ENERGY IN THE EARTH'S ATMOSPHERE	424
ON THE QUESTION OF CURRENTS OF AIR	439
ON THE GENERAL SYSTEM OF WINDS ON THE EARTH	444
ON THE QUESTION OF CAUSES OF ATMOSPHERIC CURRENTS	454

THE
SCIENTIFIC PAPERS & ADDRESSES
OF
WERNER VON SIEMENS.

ON THE EMPLOYMENT OF HEATED AIR AS A
MOTIVE POWER.*

AN engine driven by heated air, which has been working most successfully at Dundee for some time, is now being much talked about in England. Being much simpler, taking up much less space, and consuming proportionately a much smaller quantity of fuel than the steam engine, it is deservedly receiving very great and general attention.

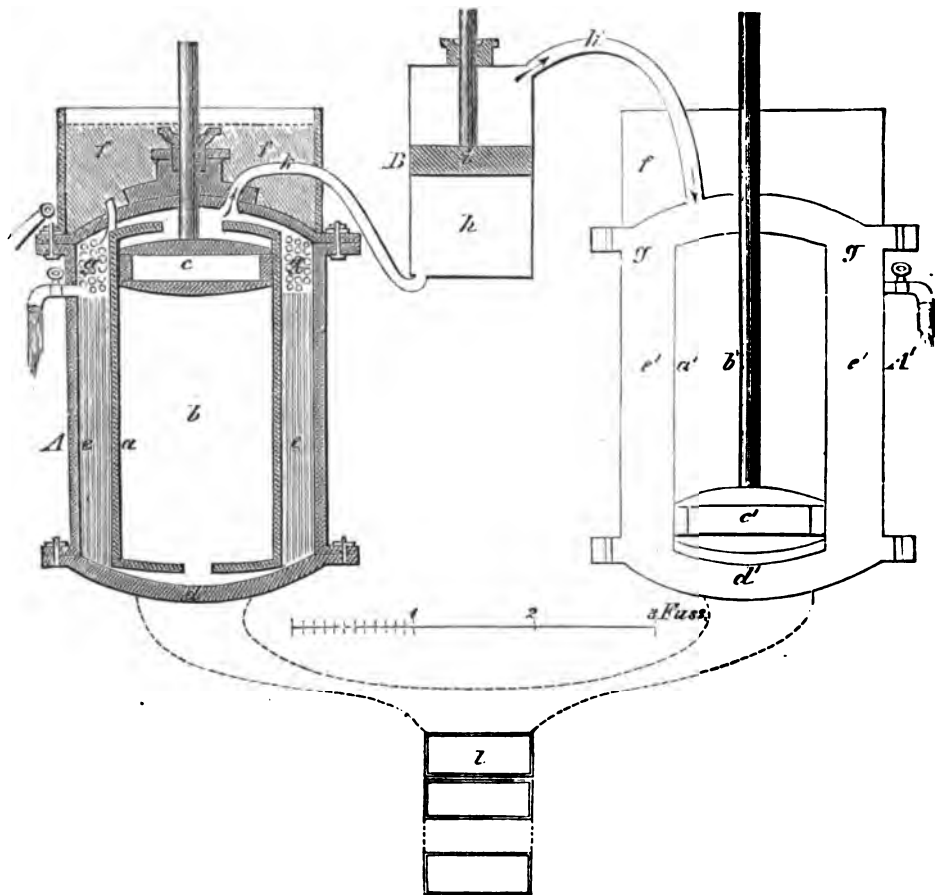
To employ, as a motive power, the great force with which confined air tends to expand when heated is not a new idea, and it has naturally received all the more attention from engineers, as the theoretically useful effect of a given quantity of heat is about three times greater when expended in heating air, than when used for the production of steam. That notwithstanding the prospect of so important a saving of fuel, a serviceable engine worked by means of heated air has not yet been brought out, may be chiefly owing to the difficulty there appears to be in quickly heating and cooling the considerable quantity of air required for working such an engine.

I have received a short written communication from England of the way in which this is accomplished in the engine above referred to, and as to how it is worked by means of change of temperature of the air. Being, however, unfortunately without

* Dingle's Polyt. Journal, 1845, Vol. XCVII., p. 324.

information in regard to the details of construction of the engine, the diagram (fig. 1) can in no way be regarded as a drawing of it,

Fig. 1.



but will serve to make its fundamental principle as clear as possible.

The engine consists essentially of three closed cylinders, A, A' and B, the covers of which are provided with stuffing-boxes. The air which at a given time is at an equal pressure in both cylinders,

A and A', is alternately heated and cooled, a corresponding increase and diminution of its expansive force takes place, and the plunger moves in the cylinder B with a pressure depending upon the difference of pressure simultaneously existing in the two cylinders.

Within each of the two cylinders A and A' there is a second smaller cylinder *a* and *a'*, in which plungers *c* *c'* move up and down; double cylinders are thus formed with an annular free space between their walls. In the top and bottom plates of the inner cylinders there are openings by means of which the air enclosed in them can freely communicate with the space between the walls of the double cylinders. If the plunger *c* is moved downwards, the air beneath it passes out by the opening *d*, ascends between the walls of the two cylinders, and returns by the upper opening into the inner cylinder to occupy the space above the plunger as it becomes empty; when the plunger moves up, the air must travel back in the opposite direction in order to get below it again. The space between the walls of the two cylinders, which must necessarily be traversed at each stroke of the plunger by the air contained in the inner cylinder, is filled for the most part with a good conductor of heat *e*, the air thus coming into contact with considerable heat-conducting surfaces. For this purpose it is best to fill the space between the two cylinders with thin concentric layers of sheet copper at small distances apart. The bottom of each of the outer cylinders A and A' is heated by means of fuel, whilst the cover is cooled by means of a reservoir of water *f* placed upon it; from the reservoir a narrow worm-tube *g* passes out, filling the upper portion of the space between the two cylinders, through which cold water continually flows.

When the plunger *c* is moved downwards, the air becomes heated by striking on the heated base-plate; but it must give up this heat to the metal plates, between which it has to pass in finely divided currents. The slight balance of heat still retained by the air after it has passed through, is taken up by the worm-tube and cold cover, so that the air becomes quite cold before entering the inner cylinder. When the plunger moves up again, the air again passes between the previously heated metal plates, but in the opposite direction, during which passage it comes into contact with hotter and hotter surfaces, and getting heated to a certain extent, reaches the hot base-plate, from which it receives a

further supply of heat. Through repeated up and down strokes of the plunger *c*, the relation between the temperature of the plates and of the air above and below them will soon become constant. The heated air gives up as much heat to the plates in the up-stroke as it receives back from them in the down-stroke. The fuel is therefore not required to supply the whole of the heat necessary for heating the cooled air at each stroke, but only the small quantity absorbed in the system of tubes, and lost by conduction, &c.

In the cover of each cylinder A and A', two tubes *k* and *k'* are fixed, which are connected to the upper and lower end of the cylinder B, so that its plunger is forced up by the expansion of the air in A, and down by that of the air in A'. Now let us suppose that plunger *c* is at the highest and plunger *c'* at the lowest point of its travel, and assume the temperature of the air in the cylinder *a* to be about 230° C., at which temperature its pressure would be about doubled. Let us suppose the cylinders to be filled with air at six times the ordinary pressure, the air contained in A would force up the plunger *i* with a pressure of twelve atmospheres, whilst that contained in A' would force it down with a pressure of six atmospheres. If now by the action of the engine itself *c* and *c'* complete their stroke, when *i* has travelled over half its course, then the force moving the latter would always attain its maximum when its velocity was greatest, whilst when it reaches its dead point *c* and *c'* arrive at the middle of their stroke. The air in the cylinders A and A' is then half heated and half cooled, and its tension in both is therefore equal, so that the plunger can get over its dead point with the assistance of the fly-wheel, as there is no one-sided force to prevent it. But as soon as it begins to move in the opposite direction, the driving force becomes active again through the continued motion of the plungers *c*, *c'*, and increasing at a very quick rate, it assures the continuous action of the engine without its being necessary to regulate the admission of the air into the driving cylinder by means of valves or slides.

As only cold air can gain admission into the upper portion of the cylinders A and A', and consequently also into the driving cylinder B, it is possible to make the packing of the stuffing-box and of the plunger *i* very perfect, even for higher pressures than are here assumed. Besides, it has been proved by experience that

it is much easier to make tight joints for air than for steam. As regards the plungers *c* and *c'*, it is much more difficult to effect a perfectly air-tight movement on account of the high temperature of the air below them. But this is not at all necessary, as the difference in pressure of the air above and below them will always be very small, namely, that corresponding to the resistance produced by the air being driven through the spaces between the plates and tubes. These plungers must however be hollow and filled with bad conductors of heat, so as not to convey much heat by conduction to the cold air above them. Any air which may escape through the stuffing-boxes can be easily restored by means of a supply pump working continuously.

It would be theoretically more correct always to fill the cylinder B with hot air ; nevertheless, the advantage previously mentioned connected with the use of cold air, namely, better packing, will certainly in all cases outweigh the disadvantage arising from it of unnecessarily enlarging the cylinders A and A', and the consequent trifling increase of fuel to produce the same driving force. It will be quite clear from what has been already said, that the consumption of fuel in this engine will be very small in comparison with that necessary for a steam engine of the same power ; the Dundee engine already referred to also fully proves this. It works up to 26 horse-power, and makes 30 revolutions a minute, with a consumption of 5 pounds of coal, whilst the steam engine of the same power formerly used consumed 26 pounds. But whilst the air is heated to 300° C. in the engine, its heat is so thoroughly abstracted from it by the system of heat conductors employed, that on arrival at the tubes it is only 8° warmer than the cooling water, and as fuel should therefore apparently only be required to warm the air to this slight degree, the consumption of fuel mentioned is still disproportionately great. This is probably due to the circumstance that in the construction of the engine, the property of air to become heated by compression has not been taken into consideration. For example, when the heated air in *a* drives up the plunger in the cylinder B, it completely fills the latter, and its density and consequently also its temperature diminish. The heat thus rendered latent cannot be absorbed by the plates, but returns with the cooled air into the cylinder *a*, where it again becomes sensible, so that by the downward motion of the plunger *i*, the

former relation of density is restored. The air already considerably heated in this way, must however pass through the convolutions of the worm-tube before it can be reheated by the metal plates. All the heat which becomes free is consequently absorbed by the cold water, and must therefore be made up for by means of firing. This considerable loss of heat can be to a great extent very easily avoided, by regulating the passage of the air by valves, so that it need pass the tubes only once, namely, as it passes up, whilst in returning it would pass round them, and so directly to the plates in its already heated condition. By this means the greater portion of the heat which has become sensible would remain effective, and the consumption of fuel would in consequence be considerably diminished.

Loss of heat cannot however be entirely avoided in this way, as the air being now delivered direct to the plates at a higher temperature, it cannot therefore completely cool the upper portion of the plates, hence the air on its return cannot be completely cooled by them, and must give up to the tubes its surplus temperature; further, the heat continually conducted up by the plates and walls of the cylinder must be continually absorbed by the cooling water, and will hence have to be restored by firing. Although the quantity of heat to be recovered may be considered very insignificant when compared with that which a steam engine requires, yet it is sufficient to make us seek means to restore it through the action of the engine itself, as for instance, by friction or continuously pumping the air into the lower, and allowing it to flow out by the upper portion of the cylinders A and A'.

Instead of atmospheric air, any kind of gas may be employed for driving the engine, and by using part of the air which has already served for the combustion of the fuel for first filling the cylinders and afterwards for pumping, and allowing it to pass over red-hot iron plates in order to free it perfectly from oxygen, the important advantage would without difficulty be gained of entirely preventing the oxidation of the heated lower portion of the cylinders A and A', at all events in the interior.

It may be anticipated that in perfecting such an engine, as in everything new, difficulties of all kinds will arise, and that all sorts of opposition will not be wanting. Although the difficulties to be overcome may at first sight appear very great, yet modern

technical science has so many resources at its command that it has already succeeded in overcoming even greater ones. The fundamental theory of the engine is too clear for there to be any doubt of its accuracy ; besides, experience has already clearly proved that no hidden error can possibly have slipped into the calculation to vitiate the result deduced from it.

When we come to consider how great an impulse industry and commerce will receive by such a reduction in the cost of mechanical power as here foreshadowed, and how great will be the gain to the whole of mankind from so important a reduction in the consumption of fuel, one cannot avoid looking upon this invention as one of the most important of the age, and joining in the hope that it may soon firmly take root, especially in Germany, where its advantages are not trammelled by any patent, and that the well-founded expectations of a grand future for it may be realised as early as possible.

APPLICATION OF THE ELECTRIC SPARK TO THE MEASUREMENT OF VELOCITY.*

A DISCUSSION has lately arisen regarding the priority of the idea of measuring the velocity of projectiles by means of the galvanic current. It appears, however, from the dates given that at a much earlier period a similar method had already been devised and applied in the Prussian Artillery. As the apparatus prepared for this purpose (which is still in use) has not yet been described in any scientific journal, although it was fully reported on at the time in some of the daily papers, I will say a few words on the origin and first carrying into execution of the idea of measuring the velocity of projectiles by means of the galvanic current, and especially by means of electromagnetism. The accuracy of these statements can be verified by the papers of the respective government departments and by official communications about this matter made to the French and Russian Ambassadors on their application.

* Poggendorff's *Annalen der Physik und Chemie*, 1845, Vol. LVI., p. 435.

On account of the great value to the artillerist of an exact determination of the initial velocity of projectiles and of the great defects which were inherent in the instruments previously used for the purpose, especially the ballistic pendulum, the Royal Artillery Committee at Berlin was induced to try an entirely new method, namely, the direct measurement of the time of flight of the projectile by means of an electromagnetic apparatus. As long ago as 1838 this method had been fully worked out by the committee in question. It consisted in constructing a clock suitable for measuring very small intervals of time, which could be started and stopped by means of magnetic force. Mr. Leonard, a clockmaker of this place, was commissioned to make it, and began in February, 1839. The great technical difficulties attending the construction of such an instrument, by means of which the thousandth part of a second was to be registered, rendered necessary important modifications of the original design and many lengthy experiments. Thanks, however, to the zeal and great skill of Mr. Leonard, this task was at last accomplished in a most satisfactory manner, and in a form which is still used in the experiments of the committee. It consists essentially of a conical pendulum, which is kept in uniform rotation by means of clockwork. By the motion of a lever a pointer can be connected to this clockwork, which is in continuous and uniform motion, and can be again disconnected from it and stopped.

In the experiments made with this clock in 1842 the attempt was made to start and stop the pointer in the following way. The ball, as it passed out of the gun, closed an electric circuit, by which an electromagnet was energised and its armature attracted. By the movement of the armature the pointer was connected to the moving clockwork, and thereby set in motion. When the ball arrived at the target a similar action took place with a second electromagnet, by which the pointer was disconnected from the clockwork and stopped.

It soon became evident, however, that the time measurements thus obtained would never reach the degree of accuracy which the construction of the clock permitted, firstly because the ball could not close the galvanic circuit directly, and for this purpose intermediate mechanism had to be inserted, which naturally introduced sources of error ; and secondly, because magnetism is not imme-

diately excited by the current, and its intensity depends on the strength of the current, and is therefore never quite constant. The interval of time that passes after the commencement of the current before the armature begins to move is, therefore, not always the same, and, moreover, the duration of the movement itself varies.

This determined me to propose already at that time to use the electric spark instead of electromagnetism for starting and stopping the pointer. This could be accomplished in various ways. The springs, through the disengaging of which the pointer was started and stopped, could be held back by exceedingly fine platinum wires, which could be successively melted by the passage of the sparks through them ; or this could be effected by means of silk threads placed in a continuous current of hydrogen, or in a space filled with oxyhydrogen gas, which would be burnt by the spark igniting the gas, or the levers used for starting and stopping the pointer could be set in motion by the direct mechanical effect of the explosion of the oxyhydrogen gas.

The Royal Artillery Committee did not, however, agree to my proposal, as it thought the insulation of long leading wires too difficult to ensure, especially when the weather was not quite favourable. They adopted instead a method of interrupting the galvanic circuit by the direct action of the ball, to which I drew their attention at the same time that I made my own method known to them, and which was suggested in the first instance by Himly, of Göttingen ; but they applied it in a way quite different from that proposed by Himly, for he intended to break the main circuit of a powerful galvanic battery and to divert the whole of the current into a branch circuit, so as to melt a fine platinum wire forming part of it, and thus start the pointer. The committee, however, retained the use of electromagnetism, but with this essential difference, that the starting and stopping of the pointer was not to be effected, as before, by the closing of the circuit, but by breaking it, and thereby causing the armatures of the electromagnets to fall.

The observations made in the summer of 1844 with the clock thus fitted up gave, on the whole, very satisfactory results, for the variable error seldom exceeded a few thousandths of a second. Nevertheless, perfectly faultless results cannot be obtained in this

way either, since the magnetic force does not cease instantaneously or even diminish considerably on breaking the circuit. This can only happen in a more or less steep curve. Hence when an armature which nearly reaches the limit of the pulling power of the magnet apparently falls at the moment the circuit is broken, in reality an interval of time must elapse before this occurs, depending on the strength of the current, as well as on the duration of its action on the closed magnet. Even when the weight of the armature is quite equal to the pulling power, it cannot fall instantaneously, since at the moment the circuit is broken the current, and consequently the attractive force of the magnet, is considerably increased through the mutual induction of the convolutions of wire.

Wheatstone and Breguet employ a rotating cylinder as a time measurer, instead of a clock, in their apparatus which has recently been made known. The armature of the electromagnet is allowed to fall direct on to the cylinder, and marks are thereby obtained on its outer surface, the perpendicular distance of which from one another measures the interval of time that has passed between the breaking of the two circuits.

It is evident that a cylinder will rotate much more regularly and quickly when connected with a conical pendulum than a pointer, which must be set in motion suddenly and must be made very light and slender, so that no perceptible disturbances may be caused owing to its mass. As the armature falls direct on to the cylinder, one mechanical link between the projectile and the time measurer is removed, and hence there is one source of error the less. But, on the other hand, there are inconveniences connected with this apparatus which make its superiority over the one here employed at least very questionable; for with it only very light armatures can be used, which are much more exposed than heavy ones to disturbing influences, both as regards the time they begin to fall and during the fall itself. But even the lightest possible armatures will at the moment of impact on the cylinder cause considerable friction and disturb its regular motion. The cylinder must be very long and proportionately heavy, and its axle must have a corresponding thickness prejudicial to uniform and quick rotation. But a much more important source of error lies in the shifting of the cylinder or of the magnets during measurement. As this

shifting motion can only start a little before the beginning of the measurement, the motion of the considerable mass which then commences, and which can only take place at the expense of the velocity of rotation of the cylinder, must necessarily occasion considerable disturbances in the regularity of the latter, which disturbances are farther increased by the considerable friction in the screw thread. The results of measurements made with such an instrument must, therefore, be very uncertain.

But although there are great drawbacks to the employment of a rotating cylinder in combination with electromagnets, it would make a very perfect time measurer if it could be made very light and short, and if it rotated quite freely.

This determined me to resume my former plan of using the electric spark for the measurement of velocity, replacing the clock by a rotating cylinder. In doing this I endeavoured to remove all mechanism between the ball and the time indicator, and to allow the spark to mark the cylinder direct. After a series of experiments with different metals and coverings I was led to decide upon a polished steel cylinder, without any covering, as the most suitable for obtaining a sharply defined and easily distinguished mark by means of an electric spark. However weak the spark may be, it produces a sharply defined and clearly visible point on polished steel. It is at first discoloured with a layer of oxide of iron, but when this is removed by wiping, a clear spot becomes visible, which under the microscope appears distinctly sunk.

The electrical chronoscope founded upon this principle is constructed as follows :—

A carefully made and divided steel cylinder, the centre of gravity of which has been exactly determined by balancing it in a bath of mercury, is kept in quick and uniform rotation by means of a conical pendulum with which it is connected by wheel work. An insulated metal point in communication with the inner coating of a charged Leyden jar is brought as near as possible to its periphery. Two metal wires pass in front of the muzzle of the gun, at a distance from each other exceeding the striking distance of the spark and are fastened to it, and they are connected to the outer coating of the Leyden jar and to the insulated cylinder respectively.

As the ball issues from the muzzle of the gun, it strikes the two wires, and its metallic mass closes the electric circuit between the cylinder and the outer coating of the jar, and the spark striking across marks the surface of the rotating cylinder. Some feet from the muzzle of the gun a second pair of wires is placed, one of which communicates with the cylinder and the other with the outer coating of a second jar, the inner coating of which, like that of the first, is connected with the point. A second spark will therefore strike the cylinder when the ball has travelled the distance intervening between the two pairs of wires and strikes the second pair; and the time taken in travelling this distance is measured by the distance between the two marks on the cylinder.

If the cylinder is divided into a thousand parts, and rotates on its axis ten times in a second, the distance of one division would represent an interval of 0.0001 of a second. By means of a vernier the tenth part of a division can be easily read with slight sparks, so that the accuracy of the measurement is increased to 0.00001 of a second. An error in the measurement of the time is therefore hardly possible, and could only arise from irregularity in the rotation of the cylinder. By a rapid rotation of the cylinder, however, the injurious effect of any error in the wheelwork is compensated, which would affect the cylinder if rotating slowly. As by this accurate measurement of the time, a movement of the projectile of $\frac{1}{1000}$ th of a foot can be read upon the cylinder, there would be no necessity to measure the time occupied during the greater portion of the whole flight which is requisite when the electromagnet is used, on account of its considerable variable error. We thus gain in many respects; in the first place the initial velocity can be directly measured, since the diminution of the velocity of the projectile in the first 5 to 10 feet will be hardly noticeable; besides this, two consecutive short portions of the path can be simultaneously measured without difficulty, and the time register thus checked. For this purpose it is sufficient to place a third pair of wires in the line of fire, which like the other two is connected with the point and a third jar. Finally there is the advantage that the intervals of time to be measured are always less than the cylinder requires to make half a revolution. On this account there is no necessity to shift the point or even the cylinder in order to ascertain the first mark or to count the number of revolutions;

and it is besides unnecessary to make the cylinder very long, or to stop it after each shot in order to read the result. The point has only to be moved slightly in the direction of the axis of the cylinder after each shot, by which means the marks are brought into a new circle, and can easily be distinguished from the earlier ones. The possibility of measuring small intervals of time with exactness renders this instrument applicable to another experiment, which will be of great importance in the theory of fire-arms, namely the measurement of the velocity of the projectile in passing through the gun itself. For this purpose it is only requisite to bore holes in the gun at different points, and to place insulated wires in the holes, communicating with the outer coatings of the jar, whilst the gun is connected up to the cylinder.

In making all these experiments, the measuring instrument can be placed in a room close to the gun, and the latter as well as the leading wires can be under cover.

There would be no difficulty in securing the insulation of the wires, as in scientific experiments of this kind tolerably favourable weather can always be waited for. At the slight distance proposed the striking of each pair of wires can hardly fail to take place, and to secure this at greater distances a frame may be used with parallel wires stretched across it arranged in the line of fire instead of a single pair of wires; the alternate wires would be connected together, so that the 1st, 3rd and 5th be connected with the cylinder, and the 2nd, 4th and 6th with the outer coating of the jar. In this way the ball will always come simultaneously into contact with two consecutive wires, and the action of the spark will thus be assured.

The instrument in the form described would however be hardly suitable for measuring the time which the shot takes to traverse long distances on account of the difficulty connected with the insulation of long wires; in this case it would be more advantageous to make use instead of the Leyden jar, of the induction coil, which could be arranged in the following manner.

An iron core formed of insulated wires is wound with two covered wires, the thicker of which completes the circuit of a powerful galvanic battery, and is placed before the muzzle of the gun, whilst the ends of the second wire, which is longer and thin, are connected with the rotating cylinder and the point, which is

brought as near as possible to the cylinder. When the ball interrupts the circuit, a spark strikes the cylinder, and even when very weak and indistinct leaves its mark upon it ; the same thing is repeated with a second induction coil when the ball at the end of its path interrupts the circuit of a second battery.

As the sensitiveness of the apparatus described may be considerably increased by very careful manufacture, exact division and quicker rotation of the cylinders, and the use of very slight sparks, the apparatus may perhaps be used with advantage to measure the velocity of electricity itself. For this purpose the cylinder must consist of two insulated discs or rings rotating on the same axis. Two points opposite to these discs are placed on exactly the same division line. If now one of these points is connected with the inner coating of a charged jar, and the connection between the two discs made by means of a long wire, then when the outer coating is connected with the second point by means of a wire of the same length, a spark will pass between both points and discs. The perpendicular distance between the two marks gives the time the spark takes in passing through half the total distance.

REMARKS ON THE ELECTROLYTIC DEPOSITION OF NICKEL AND COBALT.*

MR. SIEMENS has drawn attention to the phenomena presented by *nickel*, when deposited by means of the electric current ; whilst the surface of other similar metallic deposits is more or less dull, that of nickel presents the appearance of a natural metallic polish. Unfortunately this lustre is only of very short duration, for if the coating of the deposited metal is allowed to increase in thickness, it is first seen to crack, and then to strip off in a number of small brilliant lamina which turn their convexity towards the plate of metal upon which the deposit has been made. Mr. Siemens has also pointed out that *cobalt* shows the peculiar property of lead, of being deposited in the condition of peroxide on the positive

* Revue scient. et indust. du Dr. Quesneville, 1846, Vol. XXVII., p. 91.

electrode of a current, to the action of which its saline solutions have been submitted in such a way as to shew in it, as in the rings of Nobili, the colours of thin layers. Advantage might be taken of this circumstance in chemistry and metallurgy to separate cobalt from nickel, which is constantly covering the negative electrode with a metallic layer presenting the above-mentioned peculiarities.

ON TELEGRAPH LINES AND APPARATUS.*

THE disturbances and total interruptions of the electric telegraph service which have hitherto been so frequent, especially on long lines, are due in a great measure to variations in the strength and duration of the electric current actuating the telegraphic apparatus, caused by the length of the line wires, which are exposed to disturbing influences of all kinds. There are two ways of getting rid of these disturbances, and so giving to the electric telegraph that degree of safety, rapidity and continual readiness which it should possess, if it is to be universally extended and applied, and is to perform the service which has hitherto been expected of it in vain. The first method is to perfect the line, and protect it as far as possible from the various disturbing influences to which it is exposed, whilst the second consists in so constructing the telegraph apparatus that considerable irregularities in the currents actuating them will not put them out of order.

The first portion of the problem, or that which refers to the line, is the subject of the present article.

I shall first endeavour to give a short summary of the causes of the disturbances which we have so often had the opportunity of observing with the overhead lines hitherto exclusively used (with the solitary exception of the new Prussian telegraph lines), and at the same time to explain the means for their removal which have lately come into partial successful use.

Up to the present time the imperfect insulation of the line wires has been a special obstacle to effecting safe and direct telegraphic communication between the extreme points of long lines. In damp

* Poggendorff's *Annalen der Phy. u. Chem.*, 1850, Vol. LXXIX., p. 481.

weather the posts carrying the wire form a connection between it and the earth. If the wire and earth are in circuit with a battery, each damp post constitutes a shunt circuit to the battery, and causes an increase of current in the portion nearest the battery, and a reduction in the further portion of the line wire. Little trouble would arise from the very considerable inequality in the strength of the current at the two ends of the line, and in the coils of the electromagnets in circuit, which is often brought about by these means on badly insulated lines of even a few miles in length, if it remained constant, but being entirely dependent on the weather at different parts of the line, and consequently always varying, it necessarily occasions constant disturbances in the messages, and in the regular working of the telegraph apparatus. It has been sought by means of rotating telegraphs to diminish this varying inequality in the strength of the current in the coils of the corresponding apparatus by dividing up the sending battery. Although the object is thus partly attained, yet on the other hand, with all the telegraphs hitherto made there arises the still greater inconvenience, that the interruption of the battery at one end of the line does not occasion the complete interruption of the current in the coils of the instrument at the other end, as the part of the battery situated there remains closed through the existing leaks.

The methods of insulation formerly employed, such as drawing the wire through glass or porcelain rings, wrapping it round with india-rubber at the point of contact, the use of a protecting roof above the pole, by which it was sought to insulate the wire from the damp pole, could only act imperfectly, for in wet weather the damp insulator formed a conducting path from the wire to earth. The glass, porcelain, or stoneware bells now used assure on the other hand the insulation in a very perfect degree. On the overhead line between Eisenach and Frankfort-on-Maine via Cassel, which is 42 miles long, erected by me in the winter of last year, porcelain bells closed at the top were used, which were so cemented to the iron stalks that the bell mouth was directed downwards; the iron stalk was screwed into the top of the wooden pole, and the wire secured to the outer surface of the bell by winding it round the upper thin portion. The inner surface of the bell thus constitutes an insulated layer between the wire and the stalk, which is always dry. The insulation of this line was so perfect,

even during the most unpropitious weather (damp snow), that with the somewhat sensitive single-needle galvanometer used no current was perceptible when at one end of the line a battery of 8 Daniell cells and the galvanometer were inserted between the line and the earth, and the other end of the line was insulated.

The more perfect, however, the insulation of aerial lines is made, the more disturbing are the effects of atmospheric electricity. This phenomenon is thus explained: in the case of imperfectly insulated lines, the charges communicated to the wire by the discharged strata of air surrounding it, or by the disturbing action of clouds approaching to or receding from it, can be got rid of by existing leaks, without passing through the coils of the magnets of the instruments at the ends of the telegraph line; further that these charging and discharging currents also occur on imperfectly insulated lines during the interruption of the circuit at one or both ends of the line, whilst with perfect insulation free electricity accumulates in the wires during the interruption, which on closing the circuit goes to earth through the magnet coils, and hence weakens the normal current of the battery at one end, and strengthens it at the other. In mountainous regions the free atmospheric electricity is a special cause of continual disturbance.

As regards the above mentioned line between Eisenach and Cassel, which, following the railway, passes from the Werra to the Fulda Valley, the watershed of which forms at the same time that of the surrounding country, a galvanometer inserted in the line without any battery indicates almost continuously tolerably powerful currents of variable strength and direction, which at noon in summer often become so strong and variable that the service of the line is thus stopped for many hours. When both ends of the line are insulated there is always evidence of a considerable charge of free electricity. These charges become considerably stronger when rain or snow falls at some portion of the line. In the last case especially, the charge of the wire is so great that sparks 1^{mm} to 2^{mm} in length can be obtained, following one another in quick succession, and each time attracting the armature of the electromagnet. The currents produced in the wires by thunderstorms are yet more intense. In the summer months, when thunder-clouds appear in the sky, the regular

action of the speaking apparatus on long lines is as a rule stopped. These phenomena are much stronger also in hilly countries than on plains. The currents occurring on the discharge of clouds are particularly strong on short lines. It does not appear possible to explain these by the freeing of the electricity accumulated in the wire through induction by the clouds, for even when the thunderstorm is some miles away from the line, with every flash a very strong current shows itself. It appears as if a portion of the current produced by the discharge into the earth itself passes through the quicker conducting wire.

Hardly a summer passes with a long overhead line that the lightning does not strike it, damaging the instruments and partly destroying the line. In the case of the above-mentioned overhead line, the lightning is successfully prevented from traversing it by placing pieces of metal at intervals, and especially towards the ends of the line, as near as possible to one another, protected by the interior of the bell from getting wet. One of them is connected with the line and the other with the earth. This arrangement offers to the electrical discharge a short passage to earth of small resistance, and hence conducts to earth the lightning traversing the wire. If the masses of metal placed near to each other are large, and the distance between them as small as possible, they also serve for the discharge of the feeble charges communicated by induction to the wires. In this way their prejudicial influence on the movements of the instruments is diminished, yet a conductive connection between the two metal masses is easily caused by the frequent sparks following in quick succession between the two points. It is hence advisable, with overhead lines in the open, to use lightning conductors at intervals of the above mentioned kind, but with somewhat greater distances between the pieces of metal in order to divert powerful discharges, and to place in the rooms large metal plates at the least possible distance from one another, to render the weak charges of the line harmless. Professor Meissner, of Brunswick, under whose direction the telegraph arrangements there are placed, has likewise brought this method into very successful application, and frequently observed that the working of the telegraphs actually in use was not hindered, whilst the narrow space between the plates employed was brightly lit up by the sparks passing between them. Although

by the precautions described the disturbing influence of atmospheric electricity is considerably diminished, it is not, however, altogether removed. Thunderstorms in particular bring with them temporary stoppages in the service of overhead lines. The greatest trouble in connection with overhead lines, and one not easily removable, is due to the situation exposing them to all sorts of external destructive influences. As regards the line from Eisenach to Frankfort-on-Maine so frequently mentioned, for a long time almost daily breakages of the wire occurred, from malice, theft, accident or natural causes, and it was only by the employment of a strong body of watchers, placed along the whole line, that it was possible to maintain a certain regularity in the service by quickly repairing damages as they arose.

This uncertainty of the telegraphic service with overhead lines was long ago the cause of a general endeavour to lay wires covered with insulating material below ground. The most extensive experiments in this direction are certainly those made by Jacobi* ; he first sought to insulate the wire by means of glass tubes, joined with india-rubber, but the tubes broke, and the connection was found not to be tight. Another attempt failed, which consisted in covering the wire throughout its entire length with india-rubber, as the line in course of time lost in great measure its original insulation. Besides, india-rubber is not suitable for insulating copper, because it is decomposed by long contact with copper, and forms a conducting connection with it. The Commission formerly existing in Prussia for carrying out tests, and for making enquiry on electric telegraphs, repeated Jacobi's experiments with certain modifications without obtaining a better result. In England and America iron and leaden pipes have been frequently used to protect the enclosed covered wires from the entrance of damp. The great expense of this method, as well as the difficulty of rendering these pipes perfectly water-tight, made it of course only available for short lines through rivers, &c. It appeared, farther, that the lead pipes closely surrounding the wire came after a time into contact with it. Perhaps the unequal expansion of lead and copper with variations of temperature, was the cause of the phenomenon.

* Pogg. Ann., Vol. LVIII., p. 409.

It appeared, in fact, as though the difficulties which militated against the insulation of the whole surface of the wire were not to be overcome except at excessive cost, when a previously unknown material, viz., gutta-percha, appeared. I received the first sample of this material in the autumn of 1846, just when I was engaged with experiments on underground wires, and at once made use of it for the purpose. It turned out that even the thinnest layer of the material when freed from moisture possessed sufficient insulating property for the above-mentioned purpose. Besides, as the quality of gutta-percha of becoming plastic and sticking together at a moderate temperature, appeared to remove the difficulty of making tight joints between the separate pieces of the covering, I was soon convinced that this substance would serve to solve the above-mentioned technical problem. I therefore set to work in conjunction with Mr. Pruckner, partner in the India-rubber and Gutta-percha goods factory of L. Fonrobert and Pruckner, and made further experiments in connection with him. The favourable results of these induced me to move the Commission already referred to to make comprehensive experiments in this direction. They undertook them, and entrusted me with the direction of the work of making an experimental wire of a mile in length, which was completed in the autumn of 1847. The insulation of the wire proved so satisfactory, notwithstanding the defective methods then employed for covering it with gutta-percha, that the extension of the line to $2\frac{1}{2}$ miles from Berlin to Gross Beeren was determined on. In the spring of 1848 this work was also completed, and the line was used for telegraphic communication between the two places; the wire was covered in Messrs. Fonrobert and Pruckner's factory. For this purpose pure gutta-percha was used, entirely freed from water by means of heated rollers. The heated mass was squeezed round the wire by means of grooved rollers. Existing faults of insulation were sought for by means of Neef's inductor, and repaired by being covered with warmed gutta-percha strip. The insulation of each wire of 700 feet long was afterwards tested by means of an extremely sensitive galvanometer, and only taken into use when the galvanometer inserted in circuit with 8 Daniell cells between the wire and the surrounding water gave no sign of deflection. For greater security the wire, when laid in the trench two feet

below the surface of the railway, was further covered with a mixture of marine glue, tar and resin. The ends of the wire were soldered together with tin, and the soldered places also insulated by being covered with strips of warmed gutta-percha. The second coating of the wire appeared necessary, as experience had shown that pure gutta-percha, after lying long in water, forms a white hydrate on the outer surface, and hence the danger arises of the insulation diminishing with time. This property was specially observable after the gutta-percha had been lying for some time in seawater. In laying down some mines in Kief harbour in the summer of 1848, in which I was associated with Professor Himly, of Kiel, the wires covered with pure gutta-percha, which were to serve for the explosion of mines of gunpowder lying in the navigable channel, became coated at the end of six months with a thin layer of white gutta-percha. The white colour disappeared when the wires had been exposed for a few days to the air. It was on this ground, and on account of the great hardness of the material, that vulcanized gutta-percha was used for all the wires manufactured later.

Several tests of the above-mentioned line from Berlin to Gross Beeren made in the spring and summer of 1848 proved that the insulation remained perfectly good, and that the gutta-percha was also unaltered. As a consequence the Commission determined on the application of these wires on the Prussian State telegraphs, and the direction of the construction was given to a former member of the same, *Regierungs- und Baurath Nottebohm*.

The above trials had shown that the method of covering the wires with gutta-percha used up to that time was very faulty. The material rolled round the wire in the form of two small bands frequently did not stick well together, and channels were thus formed, which in course of time allowed the dampness of the ground to get to the wire. It also happened that after a time the seams lost their original adhesiveness, and could be easily separated from one another, and thus the permanent insulation of the wires became endangered. I therefore, conjointly with *Mr. Halske*, designed a machine, by means of which gutta-percha was continuously passed round the wire without any seam. It consists of a cylinder filled with heated gutta-percha, prevented from cooling by means of a steam jacket. A powerful screw, to which a steam

engine gives a slow forward motion, forces a suitable die down into the cylinder. The bottom of the cylinder is closed by a rectangular hollowed out metal piece, the hollow of which communicates with the inside of the cylinder. This metal piece has nine perpendicular holes bored through it, lying near to each other in a straight line. The diameter of the holes on the under side of the metal piece coincides with the thickness of the wires to be covered. The plastic material forced with great pressure into the cylinder fills the inner space of the metal piece described, and squirts out through the holes formed in it. The wires pass through the lower narrow holes into the space filled with gutta-percha, and come out covered with gutta-percha through the upper wider ones. They are then carried straight up high enough for the gutta-percha during its passage to cool sufficiently, and are then wound on drums. The later operations of testing for faulty places, and testing the insulation of the completed wires have already been described. The second covering of the wires when laid in the trenches as originally practised was not necessary with vulcanized gutta-percha, because this material has not the quality of changing into the hydrate. In fact the wires laid in the ground one and a half years ago without a second covering still remain entirely unchanged, and cannot be distinguished from newly made wires.

Wherever the wire cannot be covered with at least two feet of earth, it is protected by iron tubes from external injury. This occurs specially in passing over bridges, and in leading the wires into stations, etc. In order to give those engaged on the work of laying the wire, an opportunity of always satisfying themselves that the wire has not been injured in the operation, a clockwork is arranged at the end from which the work starts, which periodically interrupts and restores communication between the conducting wire and the earth. By inserting a galvanometer and galvanic battery between the wire and earth, the quality of the wire so far as laid can be concluded from the deflection of the needle, at the place where the work is going on.

Notwithstanding all the caution employed, it frequently happens, however, that the covering of the wires receives slight injuries in transport or in the work of laying. Such damage as consists of fine incisions, rents, or rubbed off places, cannot be directly discovered and repaired, especially when the work is executed in

dry weather. Hence, after a certain time, when through heavy rain the earth surrounding the wires becomes saturated, the line must as a rule be tested, and existing leaks sought out and repaired. It sometimes, though seldom, happens with old lines that by careless digging the covering of the wire is damaged or even the conductor itself is destroyed.

The following is the process I employ for searching for damaged places in the line.

If the conductivity of the wire itself between two adjoining telegraph stations is not interrupted, but its covering is damaged somewhere, the site of the injury can be ascertained approximately by calculation.

We may assume as known or previously obtained by experiment the length of the wire between the stations from which the site of the injury has to be ascertained; the resistance of the battery made use of, and of the two galvanometers used for the measurement, the readings of which must be comparable; the resistance of the wire which is in conducting connection with the corresponding earthplates, placed in water or damp ground, and the surface resistance of the fluid layers surrounding these plates.

All resistances are expressed in terms of the resistance of the wire.

Let x and y be the resistances of the portions of the conducting wire from the extremities A and B to the fault; m the reduced sum of the resistances of the galvanometer and battery inserted at A, of the connecting wire with the earth plate, and of the above defined surface resistance of the plate; n the same sum for the end B of the conductor.

Further let z be the resistance to the passage of the current from the uncovered portion of the wire to earth, or the resistance of the fault.

Lastly, let s be the measured or calculated strength of the current from the batteries at A and B, each of which has the electromotive force e , passing through the uninjured conductor, s' the measured strength of the current from the battery inserted at A, when the conductor is insulated at B, s'' the strength of the current measured at B, when on the other hand, the conductor is insulated at A, then—

$$\frac{2e}{x+y+m+n} = s, \quad \frac{e}{m+x+z} = s', \quad \frac{e}{n+y+z} = s''.$$

From these three equations by elimination of e and z we obtain—

$$s \cdot s'' (x + y + m + n) - 2s' \cdot s'' (m + x) =$$

$$s \cdot s' (x + y + m + n) - 2s' \cdot s'' (n + y)$$

$$\text{whence } \frac{x+m}{y+n} = \frac{2s' s'' - s \cdot s' + s \cdot s''}{2s' s'' - s \cdot s' + s \cdot s'}.$$

As the sum $x + y$, being equal to the length of the line, is known, this equation gives directly the position of the fault.

In making the measurements of the strengths of current at A and B, the precaution must be observed of always joining up the batteries between the conductor and the earth plate, so that the considerable polarization of the wire at the fault shall be in the same direction, and of taking the first reading when the polarization has reached its maximum, and the deflection of the needle has hence become as constant as possible.

Another way of testing the position of a fault gives more exact results; by this method polarization is much less troublesome, and it is independent of the electromotive force of the batteries employed.

Let the letters x, y, m, n , and z have the same meanings as given above. Further let s and s' be the strengths of the current measured at A and B, when the battery is inserted at A, whilst that at B is replaced by a metal wire of the same resistance, and connection is made with the earth plate.

Further let σ and σ' be the strengths of the current simultaneously measured at B and A, when the battery at B is inserted and the one at A replaced by an equal resistance; then as in the branch circuits the strength of the current is inversely as the resistance of the branches,

$$s' = \frac{z}{y + n + z} \cdot s, \text{ whence } \frac{s}{s'} = \frac{y + n + z}{z}$$

$$\text{or } \frac{s - s'}{s'} = \frac{y + n}{z} \quad . \quad . \quad . \quad (1)$$

Further, for the same reason,

$$\sigma' = \frac{z}{x + m + z} \cdot \sigma$$

$$\text{and also } \frac{\sigma - \sigma'}{\sigma'} = \frac{x + m}{z} \quad . \quad . \quad . \quad (2)$$

Dividing equation (2) by equation (1) gives—

$$\frac{x+m}{y+n} = \frac{(\sigma - \sigma') s'}{(s - s') \sigma'}$$

from which the fault can be localised.

It is hardly necessary to mention that the formulæ just given for determining the position of faults in lines are only applicable when there is only one such place between the points from which the measurement is taken.

It can easily be proved whether this is so or not by repeating the measurements, with a known resistance inserted at one end of the line, as calculation will give the same position of the fault, only when one leak exists. By the usual method of inserting known resistances, and repeatedly measuring at the same time the strengths of the current at the two ends of the conducting wire, the necessary data are obtained for the simultaneous determination of the position of two or more leaks, and the control of their correctness ; but the formulæ are too troublesome for practical application, and their results uncertain. It is therefore as a rule more judicious where the existence of several faults is to be inferred, either to undertake the same determinations for certain sections of the line, or to seek out the faults directly by the method of continual division described below.

As regards the constants denoted by m and n , it may be mentioned that they can be altogether neglected in the approximate determination of the position of a fault in an extensive telegraph line, which is here principally contemplated, without great prejudice to its accuracy, when large earth plates lying in water and a battery and galvanometer of low resistance are used. With earth plates lying in damp ground, the resistance to the passage of electricity from the plates to the earth which serves as an unlimited damp conductor, is naturally immensely greater, yet when at both ends there are similar and similarly situated plates, half the measured earth resistance may be assumed for each without harm. Otherwise the surface resistance must be determined for each separate plate, with the help of a third, lying sufficiently removed from both.

In order to find out faults existing in the covering of the wire as quickly as possible by continual division of the line, I proceed in the following manner :—

The ends of the line are insulated. The workmen engaged in the determination and repair of the faults are furnished with a sufficiently sensitive galvanometer, a portable battery, and a metal plate. By cutting the wire at a chosen spot and inserting the galvanometer and the battery between one end and the earth, they learn in which portion of the line the fault is to be sought. If only one fault exists, and its position is approximately obtained by calculation, they make the first trial at the calculated place. They join up and insulate the wire again, as formerly described, make a second similar test at a certain distance from this spot, continuing until they have passed the fault. They then halve the piece of wire between the two last places and so forth until the fault is limited to a few yards. This piece of wire is then laid bare, and the fault found in it repaired. To make the wire more easy of access for this test, new lines when laid down are covered with a flat stone, exactly opposite to each station stone of the railway, over which the earth is filled in. Skilled workmen only require a few minutes to apply such a test, the repair of the faulty conductor is therefore very quickly effected.

If the probable position of the fault cannot be settled by calculation, the workmen must make use of the railway trains to find between which stations the fault is to be sought. The time the trains stop is quite sufficient for the application of a test, and the first approximation is quickly made. In ten to fifteen trials the fault is discovered, even in unfavourable cases. If the workmen can employ a trolley to travel more quickly, a few hours suffice for finding out and repairing the fault between two railway stations, or say a distance of two or three miles.

If the conductor is itself interrupted, its repair by means of the division method described can be accomplished even more quickly, for it is unnecessary to cut the wire. One end of the wire is insulated, and between the other end and the earth a strong battery inserted. The workmen need now only lay bare the wire, and stick a fine needle through the gutta-percha, so that the end of it is in metallic connection with the conductor. They then know by touching the needle with the tongue, whether the wire between the test place and the inserted battery is interrupted or not. If the needle is sufficiently fine, the hole completely closes of itself. In other cases the outer covering of the gutta-percha must be some-

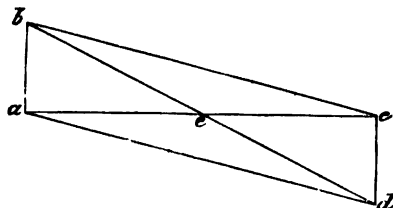
what heated to close the opening. The examination can thus proceed in many places simultaneously, and can therefore be very quickly completed.

The insulation of the conductor is now effected very perfectly. In newly established lines the leakage current must not amount in a ten-mile conductor open at the other end to more than $2\frac{1}{2}$ per cent. of the current with closed circuit; the reduced resistance of the leakages allowed in a length of a mile must thus correspond to that of a line about 400 miles in length. Such a leakage is quite harmless even to the most sensitive apparatus, for it is constant and not always varying, as with overhead lines. As underground lines are, besides, almost altogether protected from the disturbing influences of atmospheric electricity by the conducting layer of earth which covers them, there only remain as variable elements the currents circulating in them owing to the discharge of the clouds surrounding them, and those induced in them by variations of the earth's magnetism, (only of importance during auroræ), which could disturb the regular service of the telegraphic apparatus employed. As these currents, however, pass through the whole line at a constant strength, they can be rendered harmless, as will be shown further on, by a judicious construction of the apparatus. Underground lines are further, owing to their position, protected from destruction through malice, theft, flashes of lightning, and accidental occurrences of all kinds. So far as experience goes, their durability may be considered as unlimited; whilst overhead lines require to be renewed every ten to fifteen years, because the wires become brittle or rust, the poles rot, and the insulators gradually break. At present the cost of underground lines does not exceed that of solidly built overhead lines, and may possibly be considerably diminished. At the present time quite 400 miles of underground lines are in constant use. Underground lines offer many interesting phenomena, to which I shall refer when their examination is concluded. One of them, which at first rendered the application of these lines specially difficult, is that the insulating covering of the wire forms a colossal Leyden jar. The wire and damp earth form the coatings of this, and it is charged by the batteries inserted between them. With long lines these currents produce strong mechanical effects, the intensity of which is nearly proportional to the length of the wire and the electro-

motive force of the battery employed, and increase with the more perfect insulation of the wire. These charging and discharging currents must not be confounded with polarization currents. On this assumption not only are the almost miraculous properties which underground wires exhibit in practical use completely explained, but by their help there has been such success in completely governing and usefully employing them. I shall frequently return to this in describing the apparatus I have constructed.

One of the most remarkable properties of underground lines is that the apparatus will work quite as quickly with a feeble battery as the overhead wires with a much stronger one, although the conductivity of the underground wire is one-fourth less. It is not difficult to account for this circumstance when the above defined

Fig. 2.



charging currents are taken into consideration, namely, that the electricity of the battery which is bound up in the wire is distributed over the whole surface, so that only a small portion of it has to overcome the total resistance of the wire.

If the resistance of the battery employed is very small compared with the resistance of the line, the potential of the pole connected with the conducting wire remains unchanged when the other end of the wire is connected to earth.

If in the above figure ac represents the conducting wire, a b the potential of the battery inserted between a and the earth, and c is connected to earth; and if b and c are joined by a straight line, the potential and therefore also the charge at each point of ac is measured by the ordinates between ac and bc .

The area of the triangle abc represents the amount of charge. If a battery of the same strength is also inserted at c between the wire and earth, so that both batteries are joined up in series, the

line cd represents the potential at the point c , and bd the curve of the potentials of the wire. The uniformly cylindrical wire is therefore charged with positive electricity from a to the centre e , and with negative thence to c . If the wire is disconnected from the battery simultaneously at a and c , the charges of opposite electricity in the wire equalise each other. If the connection is simultaneously restored, at first there is a current of greater strength, because the charging currents have to overcome a considerably smaller resistance. By the quick succession of breaks and makes, which take place in telegraph apparatus, it is clear that the batteries employed give a greater mechanical effect with underground lines.

MEMOIR ON THE ELECTRIC TELEGRAPH.*

THE object of this memoir is to make known the electric telegraph systems which I have invented. These have been adopted by the Prussian Government from the commencement of 1848, and have since come into almost general use throughout the whole of the north of Germany.

Every electric telegraph consists essentially of two parts, viz., the conductor and the apparatus intended to transmit and receive signals. I shall consequently divide this memoir into two chapters, the first treating of the setting up of the line, and the second of the construction of the apparatus.

CHAPTER I.

1. ON THE SETTING UP OF THE TELEGRAPH LINE.

General Remarks.—All who have been engaged in the practical application of the electric telegraph will readily agree that the vast majority of the disturbances to which electric telegraphs are subject arise from variations in the strength of the currents

* Presented to the Académie des Sciences, 15 April, 1850.

employed. The cause of these variations is to be found either in the source of the currents or in the variable state of the conductor. The first of these disturbing causes can easily be eliminated by making use of constant sources of electricity. In this respect I shall content myself with remarking that I give the preference to the Daniell battery. As regards the disturbances due to the variable conditions of the line itself, three classes may be distinguished:—

1. *Losses of electricity due to defective insulation of the conducting wire.*—When the conducting wire is not well insulated, owing, for instance, to the dampness of the posts and of the intervening insulators, each current passing from the wire to the ground gives rise to a derived current, which returns to the battery without reaching the other end of the line. The strength of this current with respect to other similar derived currents, and to the main current, is in the inverse ratio of the resistances of the different derived and main circuits. It results from this that the strength of the current is increased at the battery station, and diminished at the opposite station. The action of the apparatus having naturally enough been generally adapted to the strength of the current at the first station, the increase of strength at this station has not hitherto attracted the attention of engineers. On the contrary, attention has been rather directed to the diminution of strength at the opposite station, which was the cause of the apparatus not working, and hence arose the name of *losses*, by which it has been usual to denote the effect (most striking at first sight) of the imperfect insulation of the wire.

It appears, indeed, as though the inconvenience resulting from these losses might be guarded against by adapting the action of the apparatus to the strength of the currents arriving at the distant station. The plan would be a good one if the losses always took place at the same points of the line, and if their sum were constant for each point; but as the insulation of different parts of the wire of overhead lines is absolutely dependent upon the state of the atmosphere in the neighbourhood of these parts, it can be seen that the expedient in question would be quite ineffectual.

2. *Disturbances by atmospheric electricity.*—There is, however, a perfectly effectual means of preventing the losses referred to.

This means, employed on many of the old telegraph lines of Germany, consists in winding the wire round the neck of a sort of bell made of glass or porcelain and fixed on the top of the poles, so that the insulation is effected by the inside of the bell, which is always under shelter and therefore dry. But in proportion as a reduction of these losses and inconveniences is effected by this means, another kind of disturbance arises of a not less serious character, the cause of which must be sought in the variable influences of atmospheric electricity. Experience, indeed, has proved three distinct kinds of disturbance of this nature. The first comprises continuous currents of variable strength and direction, which occur in fine weather, and especially in hilly country. In mountainous countries and at certain hours of the day these currents, the cause of which is very obscure, attain so great a strength as to oppose an insurmountable barrier to the working of the apparatus. The second kind of disturbance is produced by the motion in the neighbourhood of the wire of clouds charged with electricity. In these motions the inductive charge of the wire happening to change, currents are also observed, which in stormy weather, and especially when rain or snow falls at one of the extremities of the line, become strong enough to interrupt the service. As regards the third kind of disturbance, it is such as arises in stormy weather from actual discharges of atmospheric electricity, which strike the wire and apparatus, and independently of this havoc, endanger the health and life of the persons on duty.

The disturbances due to atmospheric electricity diminish as the insulation becomes less perfect, because at the time when the apparatus is at work and the circuit is not closed, the charges and discharges of the wire take place at the points of derivation occurring in its length, so as to free the apparatus of a portion of the extra currents ; but evidently from what has preceded it is always necessary to choose between the inconveniences arising from this cause and those resulting from losses of electricity.

3. *Disturbances due to breakages of the wire arising either accidentally or maliciously.*—I think I may limit myself to simply pointing out this third class of interruptions to which, as everyone knows, overhead wires are so subject, on account of their exposed situation. These disturbances render the use of electric telegraphs

insecure just when they are called upon to render the most important services.

General considerations regarding overhead and underground lines.—All these combined inconveniences having occurred quite early in the use of overhead wires, it is natural that to obviate them the idea soon arose of placing the wires underground. In fact, there is no necessity to point out to what an extent the safety of the service has been increased by this means, underground wires being almost entirely secure from accidental injuries and those arising from malice. In the same way it may be seen that the existence of a layer of damp, and therefore conducting, earth of greater or less thickness, which covers them, secures underground lines from injury by lightning and other sources of atmospheric electricity, which, although less violent, are even more prejudicial to the safety of the service on account of their greater frequency. Unfortunately, in opposition to these indisputable advantages, there has arisen from the very beginning the apparent impossibility of obtaining a sufficiently perfect insulation of underground wires. Hence since the origin of the electric telegraph numerous efforts have been directed to this end, which for the most part have been ineffectual. The difficulty has, however, been completely overcome in the end, and I shall now proceed to trace in a few words the history of this important advance in electric telegraphy.

History of the invention of underground lines.—Mr. Jacobi, of St. Petersburg, is the first who occupied himself successfully with the laying of underground lines. For this purpose he first tried to place the wires in glass tubes joined end to end; he then sought to cover them with india-rubber in narrow strips, which he wrapped round them. But both ways proved unsuccessful. In England and in the United States of America tubes of cast iron or lead were used for short lines, to protect the coating of varnished cotton with which the wires were covered, against the dampness of the soil; yet a sufficient degree of insulation was not attained.

Things would doubtless have remained in this state for a long time if at this period industry had not been enriched with a new material, the insulating power of which is only equalled by its wonderful aptness for being formed into the

most various shapes under the influence of heat. It is easily understood that I refer to *gutta-percha*; and, in fact, no sooner had I tested the first samples than I felt sure of the advantage that would be derived from the use of this substance in solving the problem of underground electric conductors.

It was in the autumn of 1846 that I began my experiments. In the spring of 1847 they were sufficiently advanced for me to propose to the Berlin Electric Telegraph Commission the adoption of a system of underground wires based on the use of *gutta-percha* as the insulating coating. The Commission entrusted me with the construction of an experimental line of $2\frac{1}{2}$ German miles (about 19 kilometres) in length in the neighbourhood of Berlin; and this first attempt having succeeded, the Commission in the spring of 1848 definitely adopted my system on the telegraph lines to be carried out over the whole extent of the Prussian monarchy, with the exception of those portions only where neither main roads nor railways existed.

Dating from this period, seven great underground telegraph lines have been established in Prussia for the service of the State, the greater part under my direction. These lines actually represent a total length of 300 German miles (about 2,500 kilometres). At the end of this summer (1850) this length will be more than doubled by the construction of new lines for the State and for railways, in addition to which the Austrian and Saxon Governments have also adopted my system of underground conductors for their telegraph lines.

Manufacture of gutta-percha covered wire.—The copper wires used are from 1·9^{mm} to 2·5^{mm} in diameter. They are covered with a layer of vulcanized *gutta-percha* of the same thickness as the wire, perfectly continuous, and above all without longitudinal joints. The following is a summary of the process employed for covering the wire with *gutta-percha*.

A metallic box in the form of a parallelepiped is perforated on one of its faces with a series of holes of the diameter of the bare wire, and on the opposite face with a corresponding series of holes of the diameter of the covered wire. The bare wires are passed through the corresponding holes in such a manner, however, as always to pass through the centre of the large holes. The box is filled with vulcanized *gutta-percha* in the plastic state, and

sufficient pressure is applied to cause it to pass out by the annular orifices between the bare wire and the wall of the box in which are the larger holes. In passing out by these orifices, the plastic mass adheres to the wire, and is drawn along with it in its passage, covering it with a coating of equal thickness throughout. The factory of Messrs. Fonrobert and Pruckner at Berlin (up to the present time the only one engaged in this industry) turns out daily about 40 kilometres of gutta-percha covered wire.

Method of ascertaining the insulation of the wire.—Whatever precautions may be used in the manufacture of the wire, it still happens at times that there are points where, owing to a slight fault in the continuity of the covering, principally due to the presence of small bubbles of air compressed in the plastic mass, the insulation is found to be more or less defective. Before using the wires it is, therefore, necessary to try and remove these imperfections. This is done in the following manner :—

The workman holds with one hand one end of an induction coil, of which the other end communicates with one extremity of the wire. The whole length of the wire is passed successively through a tub filled with acidulated water, in which the workman has his other hand plunged. Induction currents are continually produced by the action of Doctor Neef's apparatus with vibrating tongue. As soon as the wire, in passing through the tub, arrives at a break of continuity of the covering, the acidulated water closes the circuit by coming in contact with the metallic wire, and the workman receives such sharp shocks as could not escape the attention even of the dullest.

As soon as the defects of insulation shown in this way have been removed by means easily conceived, the wire is submitted to a last test, which consists in simultaneously immersing its whole length (the two ends excepted) in a tub of acidulated water, which is connected to one end of an astatic galvanometer of 12,000 turns, the other end of which is connected through a battery of 8 Daniell cells with one end of the wire. The least defect of insulation still existing in the wire is at once shown by the deflection of the galvanometer needle.

The laying of underground lines.—The wires are placed, without any artificial bed, in an open trench on the level of the railway at a depth of 0·8 metre. Care is taken to solder the ends of each

300 metres length of the wire, and to surround the soldered joints with gutta-percha. Bridges are crossed by means of iron tubes. Similar conduits are also used wherever, owing to special circumstances, it is requisite to place the wire close to the surface of the ground. If water has to be crossed where there are no bridges, or where there are only drawbridges, the same practice is followed, only the tubes are joined at their extremities, reminding one of the submerged "lobstertail" aqueduct of the illustrious Scotch engineer.

Method of testing the insulation and continuity of the wire.—

As the wire is exposed to many risks of accident it is necessary to make sure from time to time, during the progress of the work, that there is no break of continuity, either of the conducting wire or of the insulating covering. This is easily done, as follows :—

At the station where the laying of the wire begins, clock-work mechanism is placed, which, every two minutes, connects the end of the wire for a few seconds with the earth. Each time the workmen reach the end of a coil, they make a permanent connection between its free extremity, a galvanometer, a battery and the earth. If the conducting wire is intact, the needle is deflected every two minutes, and if the insulation is perfect, it should during the intervals return to zero.

Method of localizing a break of continuity, whether of the insulating covering or of the conducting wire.—Notwithstanding all these precautions it may happen that faults of insulation or conduction more or less considerable may develop in course of time on an underground line most carefully laid down at first. These may either be breaks in the covering, made in transport, or in the laying of the wire, which give access gradually to the moisture of the soil, or injuries caused by the pick-axes of workmen working carelessly on the line of railway in the neighbourhood of the wire, or injuries caused maliciously. The last two causes may even bring about a total rupture of the wire. It is therefore necessary to find means of discovering, as quickly and as easily as possible, the exact locality of both kinds of injury.

As regards faults of insulation, the operation may be much abridged by means of a formula I am about to indicate. Let A

and B be the telegraph stations, between which there is a fault in insulation. We will call A and B the ends of the wire which are at the stations A and B respectively. Also let a and b be the resistances of the wire comprised between the stations A and B and the point of injury, α and β the resistances opposed to the current in passing from the wire to the ground through the metal plates buried at the stations A and B, and γ the resistance to the current in passing from the wire to the earth at the fault. Then if the end B of the wire is put direct to earth, and the end A through the medium of a battery, and if s and s' are the strengths of the currents measured at A and B by means of comparable galvanometers, we shall have—

$$s' = \left(\frac{\gamma}{b + \beta + \gamma} \right) s$$

whence is obtained—

$$\frac{b + \beta}{\gamma} = \frac{s - s'}{s}$$

Now reverse the connections so that the extremity A communicates directly with the earth, and the extremity B with the battery. On passing the current through the wire in the opposite direction, so that the polarization at γ may have the same value as before, and calling σ and σ' the new strengths of the current at A and B, we shall have this time—

$$\frac{a + \alpha}{\gamma} = \frac{\sigma - \sigma'}{\sigma}$$

By dividing the second equation by the first, γ is eliminated, and we have—

$$\frac{a + \alpha}{b + \beta} = \frac{(\sigma - \sigma') s'}{(s - s') \sigma}$$

from which may be deduced the ratio of a and b . In this formula the resistance of the battery has not been taken into account, but on telegraph lines of any considerable length this resistance is so small compared with the other resistances that it may be neglected without inconvenience. The same consideration will almost always apply to the constants α and β , whose sum amounts to

what is usually called the resistance of the earth, otherwise it will be necessary to determine α and β by preliminary experiments.

Although measurements of this kind are necessarily very imperfect, still the formula I have just given serves to determine the position of a break in the insulating covering within a hundredth part of the length of even short lines, which are otherwise well insulated. In this way it is possible at least to discover between which two stations on a line of railway the fault exists. To localise the fault within narrower limits, it is necessary to proceed in the following manner.

The ends A and B of the wire being insulated, the wire is cut at a spot midway between the two stations on the railway, and the two ends are connected successively with earth through a battery and galvanometer of suitable sensitiveness. Evidently the fault will be found on that side of the cut on which a movement of the needle is observed. Hence the length of the wire which contains the fault is reduced by one-half. After having repaired the wire, the same operation is repeated at the middle point between the new limits, and so on. Twelve such bisections between two railway stations of the average distance in Germany (20 kilometres) suffice to localise the position of the fault within a few metres. Then there is nothing to do but to dig up that length of the wire, and to repair the covering by suitable means.

To determine the place of rupture of the metallic conductor, a battery is set up connected with the wire at one of the telegraph stations and earth respectively. To ascertain whether the circuit is perfect between a given place and the battery, a needle may be plunged into the insulating covering till it touches the metallic conductor, and by applying the tongue it is easy to recognise the presence of the current by the peculiar taste which is noticeable. Although this method suffices, of course use may be made of a galvanometer.

If care has been taken during the laying of the line to establish at different parts points of easy access to the underground wire, and if the men are carried by means of a trolley to hasten their travelling, two hours suffice to renew the insulating covering of the wire, on a length of 20 kilometres.

Cost of underground lines.—The price of gutta-percha covered wire, as used on the Prussian Government lines, is at Berlin about

400 francs per kilometre weighing 50 kilogrammes. For railway telegraph lines it is sufficient to use a wire which does not weigh more than half this, and which does not cost more than 200 francs per kilometre. Laying the wire in North Germany costs from 80 to 100 francs a kilometre, an expense which should, however, be distributed equally over the number of wires laid at one time.

Advantages of underground wires.—The expense then of underground wires when laid generally exceeds that of overhead wires. Besides this objection another may be mentioned, namely, that to lay additional wires on a telegraph line already existing a new trench has to be dug along the whole length of the line, whereas, in the overhead system, the same posts serve to increase at pleasure the number of suspended wires within certain limits.

Notwithstanding this, the advantage, as will be seen, is decidedly in favour of the underground system, even in point of cost.

Overhead lines are always subject to two causes of deterioration which necessitate their renewal at longer or shorter intervals of time. One of these causes is found in the rotting of the posts continually exposed to all the variations of weather; the other in the molecular change which occurs in the wires, either owing to the incessant transmission of electric currents, or to the strain to which they are submitted, and the vibrations resulting with every current of air. Owing to this alteration, the wires become brittle after a certain time even to the point of breaking with slight gusts of wind, especially during severe cold. This accident happening almost daily at different points of an extensive line renders a renewal of the wires indispensable.

Underground wires on the contrary, during the three years they have been laid, have not yet sustained the least appreciable alteration on their surface, from which it may be concluded that an almost indefinite period would pass before any alteration with which they might be threatened would affect the metallic wire. They are not liable to breakage, even when they have become brittle by the effect of the transmission of currents, because they are not subjected to any mechanical strain. The continuance of the service on underground lines being thus assured, whilst that of overhead wires is confined within narrow limits, it is evident that in the long run the former are cheaper than the latter.

As regards the safety of the service, it is evident that the same

injuries, which after a certain time make the renewal of overhead lines absolutely necessary, commence by affecting the regularity of the communications, and that on this account underground lines offer very superior security ; whilst overhead lines are exposed to all kinds of accidents, as well as malicious attacks, underground lines, almost entirely sheltered from the former, easily escape also the latter, although the route they traverse along the railway lines and main roads is known to aggressors. Further, although the insulation of underground wires is perhaps never so perfect as that of overhead lines, suspended by means of insulators under favourable atmospheric conditions, it is on the other hand quite exempt from the vicissitudes to which the insulation of overhead lines is so subject. Now, as was said at the beginning of the chapter, that is exactly the essential point. Therefore it is almost as frequent an occurrence to find electric telegraphs on overhead lines rendered useless from the effects of a heavy summer shower, or a good fall of snow, as was formerly the case with visual telegraphs. Underground lines, on the contrary, do not even offer traces of similar influences, and work in all weathers, summer and winter, with the regularity which was formerly hoped for from electric telegraphs, a hope to which the system of overhead wires has so little responded. Besides, as might have been expected, the working of underground telegraphs has been very seldom impeded by the influences of atmospheric electricity, the third great class of disturbances, which, as seen above, completes the embarrassment which endangers the security of the telegraphic service with overhead lines. Neither currents of atmospheric electricity with a clear sky, nor currents induced by the motion of electric clouds, nor yet sudden and injurious discharges during stormy weather, have any effect on underground lines, on account of the conducting bed of moist earth which covers them. For underground wires, in fact, the only disturbances that remain are currents arising from the return shock, which occasionally happen in the circuit in stormy weather or at the moment of a heavy discharge.

Remarkable phenomena presented by underground conductors.—

The following is a very remarkable phenomenon which may constantly be observed on long well insulated telegraph lines. Suppose the extremity B of the wire to be insulated, and that the other end A is connected up to a battery, the other pole of which is to earth.

At the moment of making connection, a current of short duration is observed in the parts of the wire which are not too far distant from the battery in the direction of the instantaneous current which would occur, if the circuit were closed by connecting the extremity B with the earth ; with perfectly insulated lines there remains no trace of this current. By suddenly replacing the battery by an inactive conductor, by means of a switch, a second instantaneous current is obtained, almost equal to that of the former in intensity, but this time in the opposite direction. Afterwards breaking all connection with the battery and earth at the A extremity, so as to keep this end insulated and connecting B with earth at the same instant, an instantaneous current is again observed of almost equal strength, and in this case again in the direction of the former, that is to say, of the continuous current of the battery on closed circuit. This last experiment can of course only be made when an underground line with a double conducting wire can be employed ; then the extremities A and B of the wire are supposed to be at the same station, the corresponding extremities of the double wire, at the opposite station, being joined up end to end, and insulated from the earth in such a manner as to form only a single circuit.

Taking into consideration only the direction of the currents, one might at first sight be inclined to assume that these phenomena are due to secondary polarities developed in the wire. But there are many facts opposed to this opinion. 1. The better insulated the wire the more decided are the phenomena. 2. The currents are of much shorter duration than those due to secondary polarities. 3. The strength of the currents is proportional to the force of the battery, and independent of the strength of the derived current, if such existed in consequence of imperfections in insulation ; it follows that the strength of the instantaneous currents may much exceed the maximum to which the strength of the current due to secondary polarities rises in the same circuit. 4. Finally, the strength of the instantaneous currents is proportional to the length of the wire, whilst an inverse relation should hold good if these currents were due to the discharge of secondary polarities.

Hence there is no use thinking of explaining the phenomenon by means of these polarities. But it may be very easily understood by recalling the beautiful experiment by which Volta furnished the most striking proof of the identity of voltaic and static elec-

tricity. The physicist of Como showed that by connecting one of the terminals of one of his batteries with the earth, and the other with the internal coating of an uninsulated Leyden jar, a charge proportional to the strength of the battery was produced almost instantaneously in the jar. There is observed at the same time in the conductor, between the battery and internal coating, an instantaneous current, which, according to Ritter, offers all the properties of an ordinary current.

Besides, it is evident that the underground wire, with its insulating covering, may be exactly compared to an immense Leyden jar. The glass jar is the gutta-percha covering; the internal coating the surface of the copper wire; and lastly, the external coating, the damp soil, which acts in this case like the hand in the first experiment of the canon of Danzig. In order to form an idea of the capacity of this new kind of condenser, it is only necessary to remember that the surface of the wire is equivalent to about seven square metres per kilometre.

By connecting one end of the wire to a battery, of which the other terminal is connected to earth, whilst the other end of the wire is insulated, the wire must take a charge of the same sign and potential as the pole of the battery which it touches. This is what occurs in the first of the instantaneous currents of which I have just pointed out the presence. In the experiment of Volta, by breaking the connection between the battery and the jar, and placing a conducting medium between the two coatings, the discharge is obtained in the ordinary manner. It is to this discharge as may easily be seen that the two instantaneous currents correspond which are observed in opposite directions at the two ends of the wire, by connecting these ends with the ground to the exclusion of the battery. It is to be understood besides, that the first instantaneous current, that by which the wire is charged, must be equally produced, although at a less intensity even when the other extremity of the wire is connected to earth. The instantaneous current then precedes the continuous, or if it is preferred, is added to it at the first instant. Besides, this instantaneous current has a much greater intensity than the continuous current, no doubt because, in the act of charging the wire, the electricity, in getting to the different portions of the wire, traverses distances the shorter, the nearer these points are to the battery.

However that may be, the phenomena, to which I draw the attention of physicists, imply in the construction of apparatus destined for underground telegraph lines, certain arrangements to which it will be necessary to refer later.

Another peculiarity of underground wires is that when there is a derived circuit due to the imperfect insulation of the wire, the derived current which exists in this circuit is always of greater strength when the wire is supplied with positive electricity by the battery than when the contrary is the case. Unfortunately, the study of this phenomenon still leaves much to be desired, on account of its being produced in a striking manner only on lines defectively insulated.

I will refer with more reserve to a third phenomenon which I believe I have noticed on underground lines,—viz. the production of currents of variable strength and direction due to variation in the elements of terrestrial magnetism, accompanying the aurora borealis. I observed the most striking case of this kind on the 18th October, 1848, on the line from Berlin to Coethen, twenty German miles long (about 150 kilometres), running from E.N.E. to W.S.W., and consequently almost normal to the magnetic meridian. At dusk a magnificent aurora borealis was observed on the horizon, and during the course of the same evening, as I learnt later from the newspapers, all the electric telegraphs in England stopped working. Overhead lines, however, appear to be equally affected by the same influences; but amongst the numerous perturbations to which these wires are liable, the currents induced by terrestrial magnetism cannot be so easily singled out.

CHAPTER II.

ON TELEGRAPHIC APPARATUS.

Division of Electric Telegraphs into two classes.—The telegraphs actually in use may be divided into two classes,—viz., 1st what I shall call *combined signal* telegraphs, and 2nd A B C or *dial* telegraphs. In telegraphs of the first kind, each signal, equivalent for instance to a letter of the alphabet, results from the combination of a certain number of simultaneous or successive elementary signals. In telegraphs of the second kind, a pointer which

moves in front of a dial by means of successive elementary movements of the same kind can be stopped at any desired point on the dial, and thus establishes correspondence.

Comparison of the two kinds of Electric Telegraphs.—If we compare these two great classes of telegraphic apparatus, it is soon seen, that as regards the essential matter of safety of service, dial telegraphs are notably preferable to those with combined signals. In fact, whilst the last demand on the part of the operators special dexterity, often very considerable, and the acquisition of which is difficult, dial telegraphs can be easily worked and are within the capabilities of every one. The signals of the dial telegraphs always consist of a coincidence of the pointer with one of the signs inscribed around the dial, and to read them a single act of attention on the part of the operator who receives the message is all that is necessary. On the contrary, combined signals require as many such acts as there are elementary signals. This kind of signal must therefore necessarily try the attention of the operators much more, and the chances of error with it are multiplied by the mean number of elementary signals which enter into the composition of a combined signal. Further, the instant the pointers of the dial instruments get out of step from any cause, the operator is made aware of the accident either by the incoherence of the despatch, or if it is in cypher, by the disagreement of the controlling signals. In combined signal telegraphs, each signal being independent of those which have preceded, the operator in receiving the despatch is not warned of any error, and this may cause the most serious inconvenience. Besides, printing the message or immediately recording it by any process would not remedy this defect, because this mode of transmission can eliminate errors in reading, but not those arising from the instruments being out of order.

Thus the superiority of the dial instruments over those of the other kind, as regards safety, is well established in principle. If, notwithstanding this, combined signal telegraphs are much more widely used, the reason is to be found in several circumstances. In the first place, the mechanism of dial instruments is generally more complicated, and therefore their price is higher. Further, these instruments do not so far appear to be susceptible of working as quickly as the combined signal telegraphs, because the time is always lost which the pointer takes to traverse that portion of the

circumference comprised between each letter and the following one. Finally, in the trials which have been hitherto made, the working of dial instruments has always been found to be exceedingly subject to all sorts of disturbances, especially those due to variations in the strength of currents, which so frequently occur on overhead lines.

In the construction of the dial instrument, of which a summary description will be given, I think I have been fortunate enough to retain all the advantages of this kind of apparatus, whilst finding means to avoid (at least in great part) its inconveniences.

Description of a new dial telegraph.—Imagine a piece of soft iron turning upon an axis which passes through its centre of gravity, and serving as armature to an electromagnet, from which a spring always tends to keep it away. When the circuit of a battery and the magnet is closed, the armature is attracted. But things are so arranged, that this very movement of the armature again opens the circuit. The spring immediately becomes preponderant and pulls back the armature; and this very movement of the armature taking place in the opposite direction to the previous one again closes the circuit. It will be understood that the same action must continue indefinitely, whence oscillations of the armature arise, which may acquire a very great velocity always proportional to the strength of the current which excites the electromagnet. These oscillations of the armature are the motive principle of my telegraph.

In fact the armature carries a lever, at the extremity of which is a pawl engaging in the teeth of a ratchet wheel. Each movement of the armature causes the wheel to move a step, which thus turns in a determined direction with a velocity proportional to the strength of the current. The axis of the wheel carries a pointer which continually travels round the dial of the instrument. Around the dial are inscribed the letters of the alphabet, or such signs as are desired, equal in number to that of the teeth of the ratchet wheel. To each oscillation of the armature, therefore, corresponds a sign traversed by the pointer of the dial.

There is no need to say that the lever carrying the pawl serves both to close and open the circuit. For this purpose this lever oscillates between the two arms of a sort of fork susceptible of a slight lateral reciprocating motion in the plane which passes

through the two prongs of the fork. This small lateral motion, in one direction, closes the circuit, by establishing contact between the corresponding arm of the fork and a conducting stop. The lateral movement of the fork in the opposite direction, on the other hand, results in the opening of the circuit by putting an end to the contact which has just been mentioned. In this direction the movement of the fork is limited by a stop of stone, and consequently insulating. In its movements to either side, the lever alternately rests on one or other arm of the fork, and displaces it now to one side and now to the other. In this way it acts so as alternately to open and close the circuit. But to insure the position of the fork at both times there is another special arrangement. The sort of lever which placed under the pawl lever carries the fork is prolonged into a spring, the end of which is fitted with a stone cut in the form of an obtuse cone. The top of this cone rests on a stone cut in the form of a roof with very obtuse angle. Each time the pawl-lever causes the position of the fork to change, the cone clears the edge of the roof, and the action of the spring which tends to cause the summit of the cone to slide on the inclined plane of the roof, presses the arm of the fork against the corresponding stop, and thus prevents the circuit from closing or opening by the action of the vibrations of the fork, before the lever, at the end of the following movement, itself effects this at the proper time.

At the opposite station of the telegraph line is an exactly similar apparatus, and the same current proceeding from two batteries joined up in the same sense at the two stations excites the electromagnets of the two apparatus. The interruption of the circuit at a single point sufficing to interrupt the current throughout the whole extent of the circuit, it is at once understood that each time the armature is attracted in apparatus A it is so also in apparatus B. But it is not less evident that the armature A cannot be attracted anew in consequence of the re-establishment of the contact at A, before the spring has also re-established the contact at B. It follows that the oscillations of the armatures at A and B must be perfectly synchronous. So also the movements of the pointers on the dials A and B must correspond exactly, and if at the commencement they have been brought into a similar position

they must at each moment of their automatic, incessant, and rapid course indicate the same letter on the dial.

To transmit signals by means of this apparatus, nothing further has to be done than to obtain the means of stopping the pointer at a given letter on both dials. This means is very simple. It is evidently sufficient for this purpose to prevent the circuit from closing again by the action of the spring of the apparatus A, when the pointer has reached the given letter, since the circuit also remaining open in the apparatus B, the current will be unable to pass, and neither of the armatures will be attracted until the spring of apparatus A has been allowed to close the circuit. For this purpose a circular key-board has been arranged around the dial, which is horizontal, of which the keys correspond to the letters on the dial. By pressing a key a peg is lowered, which comes in contact with an arm fixed to the axis of the ratchet wheel parallel to the pointer of the dial. The wheel is thus stopped exactly in the middle of the path it was about to traverse under the action of the spring; hence the pawl lever remains suspended between the arms of the fork, and the circuit cannot close again by the action of the spring until the obstacle has been removed by removing the finger from the key. During this time there is nothing at the other station to prevent the ratchet wheel from accomplishing its entire path, and the spring from closing the circuit; but the circuit being open at A the armature is not attracted anew, and the needle at B will then stop at the desired letter a moment after that of apparatus A. Hence at each station there is a dial, on which, whilst signalling is going on, a pointer is incessantly going round, which each of the stations may stop at pleasure at any division of the dial; the pointer on the dial of the other station stops almost at the same moment at the same division.

Signal bell and method of signalling.—A signal bell is fitted to each of my telegraphs, of which the construction and action almost exactly agree with those of the telegraphic apparatus, with this solitary exception, that the lever which carries the armature is no longer employed to move the ratchet wheel, but the oscillations of this lever are used directly to strike repeated blows on the gong.

At times of rest, when it is not wished to signal, the circuit

between the two stations A and B is formed solely of the conducting wire, the earth, and at each station of the bobbins of the bells, the antagonistic spring of which keeps the circuit closed. When station A wishes to speak to station B, he cuts his bell out of the circuit and puts in a battery and the telegraph apparatus. Then the telegraph apparatus remains motionless, but the bell at station B gives the alarm.

After what has been written above regarding the coincidence necessary in the movements of the pointers of a pair of my apparatus, that is to say in the oscillations of their armatures, it may appear strange that two similar apparatus, the telegraph and bell, may exist on the same circuit, the one working and the other not. To understand this phenomenon it is necessary to remember the fact, that the temporary magnetism of soft iron is not fully developed by the action of a current until after a certain time has elapsed. Now, let us imagine that in the two apparatus installed on the same circuit, the antagonistic spring of one, A, is out of all proportion stronger or more strained than that of apparatus B. Then when the armature of B has already been attracted, the magnet of A will perhaps only have acquired the necessary strength to be in equilibrium with the spring; and the circuit being open at B by the movement of the armature, it is no longer possible in this case for the magnet A ever to acquire this force. The armature of A will therefore be compelled to remain at rest, and the circuit be constantly closed on this side; it follows that the apparatus B will move alone. A similar discordance may be produced also from other causes, which will be referred to later. It is easy to guess the means of remedying them. It suffices for this purpose to give the springs of the two apparatus suitable tensions by means of a screw accessible from outside. But in the bells the contrary has been done; advantage has been taken of the possibility of such a disagreement to place in the same circuit the telegraph of the station A, which wants to speak, and the bell of station B, the operator at which has to be called up. For this purpose the spring of the bell has been made weaker than that of the telegraph, so that both apparatus being inserted at the same time in the circuit, the former already works quickly by the action of the battery of the other station, whilst the latter under these circumstances still continues motionless.

It is easy to understand the advantage of this arrangement. In fact to bring about the signalling, station B, warned by the bell, cuts his own bell out of circuit and replaces it by his instrument and battery; then the two telegraphs work together. This could not take place if station A in ringing up had not previously introduced his telegraph into the circuit, and he would not have been able to do it without the pointers of the two telegraphs getting out of step with one another in consequence, unless his telegraph had remained motionless whilst the bell of the other station rang.

It follows that all these operations, which at first sight might appear complicated, are done simply by giving different positions to the lever of a commutator. Before commencing to signal the stations reciprocally assure themselves that their pointers work in correspondence by an agreed-upon signal, which consists in marking the blanks of the dials. If the pointers get out of step, they are reset by means of an arrangement permitting of the movement of the pointer over its dial by causing the armature to oscillate on open circuit by successive pressures made on a button.

Strength of the currents used to cause the new dial telegraph to work.—I consider my telegraphs to be working normally when a pointer traverses the half circumference in a second, say fifteen telegraphic signals. To obtain this velocity without taking the external resistances into account, I make use of a battery of five Daniell cells for each apparatus. But the number of cells necessary is far from increasing in proportion to the length of the telegraphic circuit between the apparatus. Hence with underground wires, the new telegraphs work very well at a distance of 50 German miles (400 kilometres) when worked at each end by a battery of twenty-five Daniell cells. Besides, use would only be made of this arrangement on lines without intermediate stations. Where such stations exist, it would be more advantageous, when it is required to correspond between the end stations, simply to bring into circuit the batteries of the intermediate stations to the exclusion of their telegraphs, rather than indefinitely increase the cells at the end stations.

Additional apparatus or relay serving to make the telegraph work at great distances.—In whatever way it may be done to make the telegraphs work satisfactorily at great distances, it will

always be necessary to augment the number of cells in a proportion which ends by involving considerable inconvenience. It is to remove these inconveniences that I provide my telegraphs in this case with an additional apparatus, which permits of employing batteries with a limited number of cells even for great distances. The principle of this apparatus is the following :—

When the circuits of the batteries of the two stations are closed, the current does not at once pass into the bobbins of the magnets of the two telegraphs, although it is forced to traverse the contact points in these two apparatus, the retaining springs of which always assure a passage for the current when they are not in action. In place of these bobbins, the current traverses that of the electromagnets of the relays, opposite to the poles of which armatures entirely similar to those of the telegraph and bell already described work on pivots. These armatures are arranged in such a manner that as soon as they are attracted they close an interruption which previously existed between a conducting stop and a lever fixed to the armatures. This interruption continues closed all the time the current passes. When the current ceases, the armatures are drawn back by springs, which contrary to the springs of the telegraphs and of the bells constantly tend to break the contact in place of maintaining it. As moreover the only work which the armatures of the relays have to do is the making and breaking of the contact, it has been possible to reduce their play considerably, and to give to their springs a very much less tension than that of the springs of the bells. Hence the least current will suffice to set the apparatus at work.

Now the instant the armatures of the magnets of the relays make the contacts referred to, the current of the corresponding battery, which previously had only to traverse the telegraph circuit, in which are comprised the bobbins of the relays, and the contact points of the telegraphs, and which on its way was strengthened by the current of the battery of the opposite station, has all of a sudden to traverse a derived circuit, which being much shorter has consequently much less resistance. In fact this new circuit (independently of the points of contact of the relays) is composed, so far as the battery of each station is concerned, only of the bobbins of the corresponding telegraph. During the whole time then that the armatures of the relays are attracted, or what comes

to the same thing, that the contacts of the telegraphs allow the current to pass, there exist for each battery two circuits of unequal resistance. One of these circuits is formed, as has been seen, of the bobbins of the telegraph ; the other is the telegraphic circuit itself, which at the other station passes at first into the bobbins of the relays and then divides into two branches—the battery on the one side, the bobbins of the telegraph on the other. It is easy to understand that the strengths of the currents in the different circuits which are open to them being in the inverse ratio of the resistances of these circuits, the bobbins of the telegraphs will be traversed by currents much more intense than if they had only been made to form a part of the telegraphic circuit with the two batteries. Thus the telegraphs simultaneously come into action by the effect of the slight current which alone traverses the whole telegraph circuit. Let us examine what happens subsequently.

The armatures of the telegraphs are attracted, and whilst in action nothing is yet changed. But as soon as they get to the end of their travel the armatures break the contact of the telegraphs, the current which excited the magnets of the relays ceases, the armature of these magnets is drawn back, and consequently the current directly derived from the battery which excited the magnets of the telegraph ceases also. The armatures of the telegraphs fall back by the pull of their springs, and cause the two pointers to take a corresponding step. Further, as these armatures at the end of their fall have reclosed the telegraph circuit for the bobbins of the relay, the same action is indefinitely continued, as in the case of telegraphs working without relays.

It follows that the current which works the magnets of the relays is sensibly diminished in strength, as soon as these magnets, by the attraction of their armatures, have closed the derived circuit of least resistance. Further it may happen that the current which remains may no longer be capable of overcoming the antagonistic springs of the relays, so that the magnets of the telegraphs never get time enough to cause their armatures to complete their course. The pointers of the telegraphs then remain stationary and the telegraph circuit closed, whilst the armatures of the relays oscillate rapidly under the sole influence of the variations in strength of the current which traverses their bobbins, variations which these armatures themselves produce by alternately closing

and opening the derived circuit. This defect can be remedied either by slackening the spring of the relays or by introducing into the telegraph circuit an auxiliary battery of appropriate strength, which is external to the derived circuit when this is arranged through the bobbins of the telegraphs.

On replacing the telegraph by the bell at one of the stations, the first remains motionless, whilst the second works; so that the method of working the bell remains the same with the relays as without them.

As the relays always retard the working of the telegraphs a little, it will be well never to have recourse to them except on lines of great extent without intermediate stations. In order to cause telegraphs to work with relays to the exclusion of resistances external to the apparatus, three Daniell cells are required on each side. With a distance of 400 kilometres between the two stations, each battery should be of six cells.

Printing Telegraph.—To each of my telegraphs a printing apparatus may be attached, which prints in ordinary type the letters corresponding to the keys which are depressed. The following is the principle of construction of the apparatus.

There is in the first place an electromagnet, an armature with its spring, a pawl-lever, a ratchet wheel, all similar to those for the telegraphs. When the bobbins of the electromagnet are introduced into the telegraph circuit, either directly, or by a method of transmission analogous to that which has been described, it is clear that the wheel will go at the same pace as that of the telegraphs. In place of the pointer, the axle of the wheel carries in this instance the type wheel of Mr. Wheatstone, divided into as many projecting sectors as there are signals on the dial, each sector carrying a stamp. As the wheel moves, the letter corresponding to that to which the pointer of the dial points at each instant is immediately above a hammer. Above the wheel is placed a blackened roller, between which and the stamp passes the slip of paper to be printed. The roller is formed of a number of discs of paper placed round the axle, similar to those of which Zamboni's dry pile is composed. This collection of discs is compressed by the hydraulic press, and the edge turned up in the lathe.

All that is now required for printing is to arrange that each time a key of the key-board of one of the telegraphs is pressed down,

the hammer should strike a blow from below upwards. In the apparatus there is a second electromagnet of great power, which we will call the printing magnet, the bobbins of which are connected to an auxiliary or local battery.

The pawl-lever oscillates as in the telegraph, above a lever furnished with a piece analogous to what we have called a fork in the telegraph. But this piece is distinguished from the fork in question in having only a single arm. It is also susceptible, as in the telegraph, of a slight lateral movement. In one of the positions due to the motion, the single arm of the fork is supported against a conducting stop. In the other direction the movement of the lever carrying the fork is limited by a stone stop. Further, the two positions of the lever are regulated as in the telegraph by a stone cone pressing with a spring on a stone rest with a very wide angle. At the part of the pawl-lever which corresponds to the fork, this lever carries on each side a stud, the one insulating, the other conducting. When the instrument is at rest the conducting stud, by the action of the antagonistic spring of the electromagnet, rests against a conducting stop; when the armature is attracted on the contrary, the lever strikes with its insulating stud the arm of the fork, and forces it into the position in which this arm is in contact with the conducting stop.

The whole of this system, of course, is no longer engaged in the circuit of the electromagnet which moves the pawl-lever, and of which the reversals of magnetism arise from the operations of the telegraph instruments; but it is the circuit of the printing magnet which it is important to open and close at the right moment by means of the system in question. There are, therefore, two contact places where this last circuit is able to be interrupted. Suppose, in fact, the arm of the fork in the position where we had left it, that is pressed against the conducting stop, and the conducting stud of the lever also in contact with the corresponding stop through the action of the spring, then the current of the auxiliary battery takes the following course. On leaving the bobbins the current enters the lever which carries the fork, passes at the place of interruption of the fork into the conducting stop, thence to the pawl-lever, leaps the second point of interruption, and so returns to the battery and bobbins.

Even if the pawl-lever is removed ever so little from the corre-

sponding stop by the action of the electromagnet in action in the telegraph circuit, the circuit of the printing magnet will be opened, and however little the arm of the fork may move away for its part from the corresponding stop, the circuit will equally be opened. At first and when the printing should begin, the fork is in this last position; the pawl-lever, on the contrary, touches its conducting stop; the circuit of the printing magnet is therefore open. As soon as the telegraph current arrives, the lever (by the attraction of the armature which carries it) drives the arm of the fork against the stop, and thus puts an end to one of the interruptions of the printing circuit. The telegraph, by re-opening the circuit of the magnet, lets the lever obey the action of the spring, the lever falls back against the conducting stop, and so the circuit of the printing magnet is again quite closed. But there is another circumstance which again prevents its action. In fact, this closing is only instantaneous, because the armature is hardly drawn back before it is attracted anew by the effect of closing the telegraph circuit. Now, to cause the printing magnet (which is not like the other electromagnets of my apparatus, composed of concentric tubes split longitudinally) to come into action, an instantaneous current does not suffice. Its magnetism, in this case, does not attain the necessary strength. But if one of the keys of the key-board of one of the telegraphs is pressed in such a manner as to keep the telegraph circuit open ever so little longer than is usual in the ordinary working of the apparatus, then the pawl-lever resting for a moment against its conducting stop, the circuit of the printing magnet continues closed sufficiently long to give the magnetism time to develop, and the armature is attracted. The following then are the various functions which in its movement this armature is called upon to perform :—

1. The hammer suspended below the letter to be printed is, as has no doubt been conjectured, fixed at the end of a lever which the armature of the printing magnet carries. By attracting this armature the hammer strikes its blow, and the letter is printed on the paper, corresponding to that which the pointer of the telegraph indicates.

2. In accordance with the distribution of the signals around the telegraph dial, two diametrically opposite sectors of the type wheel remain empty. Then when the hammer has struck one of

these empty places, the armature can describe an angle a little greater than in the case of the full ones, where the stamp immediately meets the printing roller. The effect of this is that another lever fixed at the other extremity of the armature is able, in the case of the empty places, to strike a clock bell and make it sound. As it is useful to leave blanks between the words of the message, at each word, by touching the blanks of the dial, one is advised by the sounding of the bell that the positions of the pointer on the dial and the type wheel above the hammer agree. If by accident this agreement fails to exist, it is always easy to re-establish it by means of an arrangement which permits of the wheel being moved, by causing the armature to oscillate on open circuit by successive pressures on a button.

3. If the circuit of the printing magnet remained closed longer than is absolutely necessary for the armature to strike its blow on the hammers, many grave inconveniences would happen. The pressure of the hammer against the roller would then be continued. The magnetism would be so highly developed in the soft iron that the magnetism would not loosen the armature quickly enough after the circuit was broken. In consequence, the hammer might catch the wheel, and if this accident did not happen, the armature would certainly not have time to fall back into its first position under the action of the spring. Now, one sees that it is in its fall that the armature causes the printing roller to advance the necessary step, and besides, if the next stroke of the hammer only started from a point in the travel of the armature, more or less distant from the magnet, sufficient energy would not be accumulated, and the two neighbouring letters on the dial could not be printed. Finally, as immediately after breaking the circuit it is liable to be closed again at short intervals, although for short spaces of time only, it might even happen that the armature would not detach itself at all from its stops.

To guard against these inconveniences, it is then of the greatest importance that the printing circuit should be opened the instant after the letter has been printed. It is for this purpose that the double interruption apparatus already described is used. In fact, at the same moment that the blow of the hammer is struck, a third lever fixed to the armature gives the fork the lateral movement necessary to withdraw it from its conducting stop, against which

it had been driven by the first motion of the pawl-lever. The printing circuit is then open, the armature of the printing magnet has full time to fall again, and when the telegraph is left to itself by removing the finger from the key, the first motion of the pawl-lever at once re-establishes the contact between the arm of the fork and the conducting stop.

4. Finally, as has been indicated, the armature of the printing magnet fulfils another indispensable office, viz., that of causing the printing wheel to turn by an angle corresponding at its circumference to the size of a letter of the type wheel. This is effected by means of a pawl-lever and a ratchet wheel conveniently arranged. The roller, in turning, draws the paper slip along, which passes between its blackened surface and the type wheel. But it will be understood that this simple displacement of the roller is not sufficient. In fact, it thence results that with each new turn of the roller which corresponds to 100 letters, including blanks, the letters would press exactly on the same places, so that not only would the black surface be soon exhausted, but the roller would be worn away in the most unequal manner possible. In order to prevent this, there is a further arrangement of such a kind that the roller is displaced by a small fraction of its length at each step of the ratchet wheel; after five turns it is displaced by almost the height of a letter. In this way it will be seen that the printing would always act on strips of the surface of the roller parallel to its axis, in such a manner that there would always remain between the strips permanently used still narrower strips which would never be used. The further precaution has therefore been taken of giving the roller a slight movement of rotation forward, so that the impressions of the hammer at each new turn of the roller no longer correspond exactly to the impressions made in the preceding turn, but continually encroach on them like the lines of a vernier on those of its scale.

Arrangement for preserving the parts of the circuit where sparking occurs from wear and tear.—All constructors of electromagnetic apparatus know but too well to what an extent the points of interruption of the circuit where the spark passes are subject to rapid deterioration by the action of currents, although only of slight strength, even when use is made of platinum. This circumstance for some time seemed to offer an insurmountable

obstacle to the regular and prolonged action of my apparatus, until I found that by replacing the platinum by an alloy of this metal and gold, coatings were obtained for the points of interruption which were almost unaffected by currents of such intensity as I employ. In fact, this alloy possesses a cohesion and hardness much greater than those of platinum, and hardly participates at all in the property of this metal of getting reduced to powder, and being carried to the negative pole by the action of the currents.

General remark on the principle of construction of the new dial telegraphs.—After having given a description of the new instruments which I have invented for the purpose of telegraphic signalling, I now propose to refer to certain considerations necessary to bring forward the principal advantages which I think belong to them.

Every process of electromagnetic telegraphy will always be reduced as a matter of fact to making suitable use, for the transmission of signals, of a series of magnetizations and demagnetizations successively effected by making and breaking a circuit. In all other dial telegraphs, the direct action one of Wheatstone included, and those constructed on the same system, the essential operation of opening and closing the circuit is placed in the hands of the person sending the message, and besides the break is only made at the station from which the message is sent. On the other hand, each of my apparatus is in itself a magnetoelectric machine with appropriate motion, so that in these instruments the current itself breaks the circuit, and that at both extremities of the line at the same time. This circumstance, which is quite special to them, involves a number of remarkable consequences, of which I shall refer to some of the most essential. Indeed, the principle of the automatic interruption of the circuit will probably acquire the same importance in electric telegraphy as the invention of that child has done in the art of constructing steam engines, who was inspired with the happy idea of making the motor itself undertake the duty which was irksome to him of opening and shutting the steam valves as required.

Advantages to be found in the principle of construction of the new telegraphs.—If after what has been said at the beginning of the first chapter, the ideal of a telegraph conductor must be considered as most nearly approached by those in which the strength of the

currents is subjected to the least possible variation, it is necessary on the other hand to consider such telegraph apparatus as the most perfect, of which the movement, without extraneous help, is least affected by variations of strength, which have still to be overcome. Now, I do not think I hazard too much in affirming, that, in this connection, thanks to the principle of automatic interruption, there are no telegraphs that can be compared to mine.

When the duty of closing and breaking the circuit is effected by an action exterior to the instrument, it is almost impossible that each time it should last only just long enough for the magnet to attract the armature. The time necessary and sufficient is smaller as the strength of the current is greater. It is true that for a given strength one can determine by experience the most convenient duration to give to the closing and opening of the circuit. But as soon as the strength of the current begins to vary, especially if of unequal magnitude at the two stations, as is often the case with overhead wires, there will be trouble again; either the closings will not last long enough for the existing strength of the current in the receiving instrument, and hence the magnet will not be able to attract the armature, or they will continue too long and then the armature may stick by the effect of temporary magnetism. In both cases the relay will outrun the receiver, and the correspondence will be disturbed. In instruments of this kind it has been necessary, in order above all to reduce the chances in favour of the last case, to reduce the masses of soft iron to the lowest proportions, because, with equal intensity of current, the temporary magnetism increases with the size of the magnet.

On the contrary, when the apparatus itself breaks the circuit at the end of the travel of the armature, it can never happen that the circuit does not remain closed long enough, the interruption always taking place at the desired point; that is to say, at the precise moment that the magnet has supplied the necessary work to cause the pointer to advance a step. On the other hand, the circuit will never remain closed too long, for the quantity of magnetism developed in the magnet will be always sensibly the same at the moment the circuit is broken, whatever the strength of the current may be, because the motion of the armature will be the more rapid, and the rupture will always take place at the moment when the magnet has acquired in more or less time, according to the strength,

a force regulated by the constant force of the spring, and consequently itself sensibly constant. As to the time of opening, with equal strength of spring it will always be sensibly the same; so that when the instrument goes too quickly under the influence of a more powerful current, the same degree of temporary magnetization will always take the same time to be effaced, and the armature will never get fixed. There will therefore be nothing further to fear from temporary magnetism, and consequently the mass of soft iron may be increased without inconvenience, which offers the advantage of allowing the work to be done with a smaller current. It is further evident that the same actions taking place in each of the two apparatus installed in the circuit, their working will continue to be synchronous, for this reason alone, whatever may be the strength of the current.

But the security in this connection considerably increases from the fact that the interruption of the circuit takes place simultaneously at both ends of the line. In fact, each of the two instruments holding, so to speak, the current locked for the other until the convenient moment, the strength of the currents may be different in the two instruments, and yet their armatures will be attracted at the same moment. The instruments will, therefore, work together up to a certain limit, which it is easy to predict. This limit will be attained when the armature of the apparatus excited by the most powerful current, on arriving at the end of its travel again opens the circuit before the armature of the other apparatus can complete its own, owing as much to the *vis viva* collected during the closing of the circuit as to the temporary magnetization of the masses of soft iron. When this limit is passed, that armature of the two telegraphs which is weaker will only make small weak oscillations, and its pointer will remain unmoved. The apparatus, however, may still be made to work together, even under these circumstances, by weakening the spring of the instrument which refuses to work.

By the same means one could further compensate, if necessary, the beginning of a similar disagreement taking place, owing to very different quality of the soft iron, or a different arrangement of the magnets of the two instruments. It would be necessary to slacken, in this case, the spring of the apparatus of which the iron had the greater coercive force, or of which the magnet

presented a continuous mass instead of being composed of concentric tubes split longitudinally. I may add further that experience has proved that the working of the telegraphs is most rapid when the strength of the current and the force of the spring are so regulated that the times of attraction and repulsion of the armature are equal.

To sum up, one sees that in telegraphs with automatic double interruption the rapidity of working of the instruments always adapting itself quite naturally to the strength of the currents, this velocity serves as a regulator, equalizing the irregularities which might arise from variations of strength. A very curious property of these telegraphs may now be understood which at first view even appears paradoxical.

Let us assume that two of these instruments need, in order to attract their armatures completely, a strength of current a . It will of course be indifferent in what manner the necessary strength a is supplied to each apparatus. Thus a local battery may be arranged at each end of the line, incapable of working the apparatus at the station alone, because it would only furnish a strength $b < a$. Then, by passing into the circuit of the two instruments a current of strength $c =$ or $> a - b$, the two instruments might be made to work together however small c might be with regard to a , provided things were so arranged that each of the instruments in working breaks at the same time the circuit of the local battery, and that of the current which traverses the entire circuit.

Now this arrangement can easily be realized. Imagine a telegraph circuit with two of my apparatus at the two stations, each apparatus being furnished with its battery, but the current derived from the two batteries incapable of working the apparatus. Then let a derived circuit be inserted at each station, between the wire passing from the battery to earth, and that passing from the telegraph to the other station; this is what will happen. In each telegraph and each battery, the current of the same battery will increase in strength, because the insertion of the derived circuit will diminish the resistance of the circuit offered to the battery. On the other hand, in each telegraph and each battery the current of the other battery will diminish in strength, because when many circuits are worked simultaneously by the same battery the

strengths are inversely as the resistances. But the increase in the current of the corresponding battery in each telegraph may be greater than the diminution in the current of the other battery, and in this manner, from the very fact of the insertion of derived currents, the strength in each of the telegraphs will become great enough for it to enter into action. However, for the pointers to keep together, it is necessary that another condition should be fulfilled. This condition is, that the current of the battery of each station in the telegraph of the same station, when it circulates in the derived circuit, shall not be great enough alone to make the telegraph work, because, if that was the case, one of the telegraphs might work without the other, since the breaking of the circuit at one of the stations would not cause the breaking of the circuit at the other station. Besides, this condition could always be easily arranged by giving sufficient tension to the antagonistic springs of the two instruments.

Let us now assume that the current of the two batteries in the telegraph circuit was alone capable of making the apparatus work, then the insertion of the derived circuits will evidently make them work more quickly. Let us further assume that the derived circuits were either not of equal resistance, or were not disposed symmetrically, or even that there is only one at one of the extremities of the line; in this case the strength of the current in the two instruments will be no longer the same; it will be increased in the apparatus to which the derived circuit of least resistance corresponds, or the only similar circuit existing, and it will be less increased or diminished in the other apparatus. Nevertheless it will be understood, after all that has preceded, that the telegraphs will work together, and with a velocity which, in this case also, may much exceed that which would be obtained in the derived circuit. The uniformity of the instruments will have, it is true, a limit, the same as has been indicated above, beyond which one of them will refuse to work; but it will be easy to re-establish the uniformity by conveniently regulating the tension of the springs.

Let us apply these principles to what actually happens on telegraph lines. All that has been said with regard to derived artificial currents, equally applies to those which, on telegraph lines, result from defective insulation of the wire. One may call

to mind that it is similar currents which, in presenting to the current of the battery a shorter road, occasion what it has been usual to call losses, because the only thing which up to the present had been observed in this phenomenon, is the weakening of the current of the opposite station. Mr. Wheatstone had tried to remedy these losses, and the variations in their amount, by establishing a battery at each station ; but with his dial telegraphs, and those of the same kind, this precaution does not succeed, because the circuit being interrupted only at one of the stations, the armature of the receiving instrument is too easily jammed by the effect of the current of the corresponding battery, which still exists in the derived circuit. And the singular thing is, these very losses, so troublesome for all other telegraph apparatus, not only do not affect, as has been seen, the regular working of my double automatic interruption telegraphs, but even favour and accelerate it, and within very wide limits, because the current established in the derived circuit, before it can disarrange the signalling, has not only to keep fixed an armature already attracted, but must become powerful enough to attract it at a distance, after it has been drawn back by the spring, and before the armature of the other apparatus has been altogether drawn back.

This remarkable property of my apparatus to work rapidly and with precision, even when there are derived circuits which would put a stop to the working of other telegraphs, acquires even greater importance for the following reason. I described towards the end of the first chapter, phenomena which result from the circumstance that the copper wire, with its insulating covering, acts as a gigantic Leyden jar, which receives its charge from the battery, with which one of its extremities is in contact. These phenomena give rise to certain derangements in the working of telegraph apparatus in general. In those of my construction they are easily the cause of one of the instruments remaining stationary whilst the other works with great rapidity. There is a very easy method of remedying these disturbances ; this method consists in establishing a derived artificial circuit from the wire which goes to the other station to the wire which goes from the battery to earth ; so that as it is only my instruments of which the working is not injured by the presence of derived circuits, so it is only by

them that all the advantages of well insulated underground conductors can be gained.

Advantages found in the mode of action of the new telegraphs.—In my system, one wire, and at each station one apparatus and one operator, suffice to give and receive signals. As many instruments as desired may be brought into the same circuit, all of which will work together. All the apparatus of each station inserted in the same circuit can be stopped together at the same instant. Hence, at every moment of the transmission of the message, when there is no printing apparatus, each employé who receives it may stop the one who is sending it, and thus gain the necessary time to note the word that he has received, without the risk that during this occupation fresh signals will escape his attention. Besides which there is nothing easier than to speak from one of the end stations to an intermediate station without the others receiving the message. At an agreed upon signal, the employés of the intermediate stations withdraw their instruments from the circuit, and replace them by a bell which remains quiet under the action of the intermittent current, but gives warning as soon as a continuous current traverses it, on the same principle which causes the printing magnet, in the apparatus described above, only to work when the circuit is kept closed for a certain time. The message finished, the two employés of the corresponding stations withdraw in turn their instruments from the circuit which made the current of the batteries intermittent, so that it becomes continuous, the bells of the intermediate stations come into play, and warn the employés that it is time to reinsert their instruments in the circuit. All these various combinations are instantaneously effected by means of a lever which has three positions: in one there is communication between the two neighbouring stations; in the second the message passes unsignalled from one station to another, to the exclusion of the telegraph instrument as has just been indicated; in the third all the telegraphs receive the same message together. Lastly, to each of my telegraphs, as has been seen above, a printing apparatus can be adapted, so that the message is printed at once at both stations. The correctness of the message is thus completely guaranteed, without there being any necessity of repeating it, and a mistake which might slip into the apparatus during transmission could never affect more than a

single word of the message, because it would be immediately discovered by the bell, which, when everything is in order, must sound between each word and the following in accordance with the blanks of the dial. The printing machine only communicating with the telegraph electrically its mechanism does not hence become more complicated, and the disorders to which the printing apparatus may be subject, on account of its greater complication, do not react on the telegraph. The working of the telegraph is quite as rapid with the printing apparatus as without it, and the printing does not even cause any loss of time in the transmission of the message, because it takes place at the moment in which the telegraph may be considered as stopped for a moment by the effect of lowering the key. Finally, as it is the roller and not the type wheel itself which carries the black, the impression is always equally black, and clear from one end of the message to the other, whatever may be its length.

This telegraph, either with or without the printing apparatus, does not require any special dexterity in use, because it suffices for this purpose merely to find a place on a key-board, and this naturally without the least complication in the apparatus arising from the use of the key-board. As to the rapidity of correspondence, an operator without being overworked gives 50 to 60 complete signals in a minute, or letters printed in ordinary characters, the blanks included. This number will not appear large compared with what other apparatus supply, for instance the electrochemical telegraph of Mr. Bain ; but it must be remembered that my telegraph does not require any preparation for working, that it is always ready for use, and that on the other hand, the message is given in ordinary characters, so that no time is lost in deciphering it.

Conclusion.—The telegraph apparatus, of which I have just described the construction and advantages, are not simply in contemplation. On the contrary, the instruments have been adopted for three years by the Prussian Government ; many railway companies have followed their example, and now more than 150 of these apparatus are at work in the north of Germany, a number which will be doubled in the course of the present year. Since they have been in use they have worked with perfect regularity, so that months have passed without the pointers getting out of step from one another.

It follows, further, that these apparatus, notwithstanding the simplicity of their principle, demand in their quality of instruments having direct motion, a clever, intelligent, and careful construction. May I be permitted, on this occasion, to make a public acknowledgment to my collaborator, Mr. J. Halske, of Berlin, to whose admirable talent I must attribute the greatest part of the success with which my efforts in this branch of physical science have perhaps been crowned.

ON FORWARDING SIMULTANEOUS MESSAGES THROUGH A TELEGRAPH CONDUCTOR.*

ALREADY, in the year 1849, I was engaged with Halske in solving the problem of simultaneously despatching through telegraph conductors a number of messages in excess of the number of wires. We proceeded at that time upon the following considerations :—

When the end of each conducting wire is connected to the end of every one of the other wires through a telegraph instrument with its attached battery, $\frac{n(n-1)}{2}$ such telegraph apparatus can be set up at either end of the n conducting wires connecting the stations A and B. If one of the apparatus brings its battery into circuit with the respective wires, a current of greater or less intensity will pass through all the conducting wires and apparatus. The problem now consisted in making the current passing through the corresponding apparatus at the other station as strong and effective as possible, and in making the currents passing through the remaining apparatus very weak, or in compensating their action entirely or to a large extent. This could be carried into effect by a suitable choice of resistances to be inserted and withdrawn with the batteries and apparatus, by local shunts to the

* Poggendorff's *Annalen der Physik und Chemie*, 1856, Vol. XCVIII., pp. 115-183.

batteries in action, and by a proper construction of the apparatus themselves.

As both the calculations and the experiments made with a small number of wires gave promise of a good result, we claimed in a patent taken out in England on the 28rd October, 1849, the simultaneous despatch of a great number of messages through combined wires. Further trials in this direction soon showed us, however, that the solution was too complicated and difficult with a great number of wires, and that the principal requisite of telegraphic arrangements, the very greatest safety, could not be satisfactorily attained.

Some years later Dr. Kruse tried at Artlenburg to solve the problem of the multiple simultaneous use of telegraph conductors in quite a different way. He made use in his experiments of a modification of our alphabetical telegraph instruments, which are based on the principle of Neeff's hammer,* consisting in combining them with a relay. This is effected by passing the line current through the spirals of the relay, and a local current through the electromagnet of the telegraph instrument, the relay alternately making and breaking the local circuit, and the telegraph instruments making and breaking the line circuit. Furnished with this contrivance, the alphabetical telegraph instruments are enabled to work safely and quickly, with very short and weak currents passing through the telegraph line and the coils of the relay.

Let us now suppose a given number of alphabetical or printing telegraph instruments placed at each end of the line. One end of all the relay windings communicates through the sliding contacts of the respective telegraph instruments with one pole of a common battery, the other pole of which is connected to earth. The second free end of each relay coil is connected with an insulated contact spring. These springs are grouped at equal distances around a contact disc. The rim of this disc upon which the springs slide is so arranged with alternate insulating and conducting spaces that only one spring at a time is in contact with a conducting space ; all the others on the contrary are in contact with insulating spaces. If the disc is turned, the springs are one after the other for a moment in conductive connection with the disc and through it

* Arch. d. sc. ph. et nat. XIV. 41.

with the line wire. If one now assumes the same arrangement to be made at both ends of the line, both batteries joined up in the same sense, and both discs revolved quite uniformly, then all the telegraph instruments will rotate together uniformly. If one of them is stopped, and hence the conducting connection of its contact spring with the battery broken for a length of time, then the corresponding one must stand still, that is, the apparatus combined with the corresponding contact spring at the other station, for no current can pass through the line whilst the spring is in contact with it. Mr. Kruse produces the uniform rotation of the two discs by providing them with teeth, and turning them by the oscillations of the telegraph magnets themselves. As there are always the same number of apparatus in motion at both ends of the line, and all rotate with exactly the same velocity, both discs must also move forward quite uniformly. If individual pairs of telegraph instruments are stopped, the velocity of rotation of the disc and consequently also of the other telegraphs is certainly thereby diminished, but the uniformity of the rotation is not disturbed.

It is evident that this ingenious combination is too complicated and unreliable for practical application; it is especially very difficult to make the relay so sensitive and quick in action that it may safely work with currents of such short duration and set the telegraph instruments in motion.

In the number of the *Leipsic Polytechnic Central-Blatt* for the second of December, Telegraph Inspector Galle describes a method of forwarding messages simultaneously in opposite directions tried by Dr. Gintl on the Prague and Vienna line by means of the Morse printing telegraph instrument. It consisted in the relay magnets being provided with two windings of wire, of which one communicated with the line wire. While the key (contact lever) was not pressed down, its back contact established the conductive connection of the free end of the coil with the earth; the conducting circuit was consequently formed by the wire, the respective coils of the two end stations and the earth. By pressing down one of the keys the connection from the coil to the earth direct was interrupted, and on the contrary established with the free pole of one of the batteries connected to earth. Hence the current of this battery passed through the line wire and the coils forming its

continuations. In order to prevent this current from magnetising the relay at the end where the battery was put in action, a second contact was simultaneously made by the same movement of the key, which sent the current of a second battery through the second winding of the magnet. This current passed through the coil in the opposite direction, and was so adjusted by a rheostat which was inserted that its magnetising power was equal and opposite to that of the line current passing through the other coil. Hence the relay magnet of the home station remained quite unmagnetised whilst the current round the magnet of the end station exercised its full power. If now at both ends of the line the same contrivance were arranged, and if at the same time both keys were depressed, and consequently all four batteries inserted, the equilibrium of the currents in both relay magnets would be disturbed and the armatures of both must be attracted. Hence each apparatus must receive the signals given by the other station, whilst simultaneously other signals proceeded from it and appeared there.

Dr. Gintl appears to have always connected up the batteries at both ends of the conducting wire in opposition, and thinks this is indispensably necessary, for later he has frequently expressed the singular opinion that the possibility of duplex working proves that two currents could pass in opposite directions through a wire without mutually diminishing or annulling each other. In the case in question it is indifferent in which sense the batteries of the two stations were inserted, as regards the magnitude of interference with the equilibrium of the magnetic actions of the currents passing through the two coils of each relay magnet. If they were in the same sense, that is, so joined up as to work like a battery with double the number of cells, then the strength of the current in the conducting wire, and the coils connected with it, is twice as great as what a single battery produces in the same circuit. If the batteries, on the contrary, are equal and opposed, they completely neutralize each other on a perfectly insulated circuit, and no current passes either through the conducting wire or the corresponding coils. In both cases the magnets were magnetized by the difference of the action of the line current and of the compensation current, consequently as strongly as by a single current. The only difference is that, in the first case, the line current—in

the second, the local compensating current—preponderates, and brings about the magnetization.

The practical results which Dr. Gintl obtained by experiments with the apparatus arranged as just described could not but be very unfavourable. Two batteries do not long remain equal unless the resistance is frequently adjusted. It is yet more difficult, if not impossible, to make and break two contacts virtually simultaneously as is necessary in Gintl's duplex method.* Further, the connection of the wire with the earth during each passage from one state of rest to the other is interrupted; the current coming from the other station is consequently arrested during this time, whence disturbances in the arrival of the despatch necessarily result. Lastly, Dr. Gintl, by joining up the batteries in opposition, has caused the inconvenience that the relay magnets were magnetized with duplex working in the direction of the compensation current with simplex working on the contrary in the direction of the line current; on each of the numerous changes between simplex and duplex the magnetism of the electro magnet must consequently be reversed, which must have the important result that frequently short signals would be lengthened and longer ones interrupted.

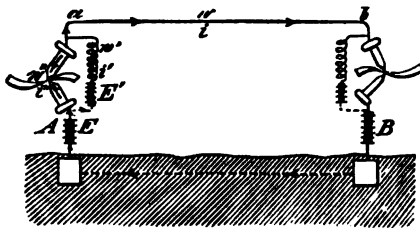
The unfavourable results which Dr. Gintl obtained with the duplex method worked electromagnetically caused him to abandon this method, and to seek the solution of the problem by means of Bain's electrochemical telegraph. In a memoir communicated on the 30th November, 1854, to the Royal Academy of Sciences of Vienna,† Dr. Gintl sought to show that two currents without mutually disturbing each other traverse the same wire in opposite directions; that consequently "each of the two currents transmitted simultaneously through the wire arrived at the opposite station, just as if it alone were passing through the wire," and founded on this supposed proof the construction of his electro-

* Dr. Gintl has himself never absolutely laid claim to the above described arrangement for simultaneously speaking through the same wire with Morse instruments as his invention. As the late Professor Petrina, of Prague, was frequently referred to as having communicated to the Austrian Government the leading idea of the experiments described, a clear statement on the matter would be very desirable.

† Report of the Mathematical and Scientific Section of the Royal Academy of Sciences, Vol. XIV., p. 400.

chemical duplex apparatus. Although this proof, as could be seen beforehand, is quite erroneous, and only shows that Dr. Gintl had left Ohm's law and the theory of derived currents out of consideration, yet the idea of duplexing electro-chemically which he first suggested is well worth considering. It will, however, be most convenient to establish the conditions of the electrochemical duplex apparatus by means of a simple calculation, and not to enter further on Gintl's proof.

Fig. 3.



In fig. 3 *a b* is the conductor, *c d* the return through the earth considered as without resistance, through which the two stations A and B communicate with each other. The connection between *a* and *c*, as well as between *b* and *d*, is made by means of paper strips steeped in some salt solution and intended for the reception of the telegraphic signals, and the circuit thereby completed. If a battery, *E*, is now joined up in circuit at one of the two stations, at A for instance, the current will traverse both paper strips and produce at both stations a decomposition of the electrolyte with which they are saturated. The problem of duplex working requires, on the contrary, that a decomposition should only take place at station B, that the paper strip at station A should consequently not be traversed by any current. This can be effected if simultaneously with the battery *E* a second battery, *E'*, together with a resistance, *w'*, yet to be ascertained, is inserted between the two metal points serving as electrodes, between which the paper strip passes in such a way that the paper strip forms the branch of a Wheatstone bridge, through which no current passes. If *E* and *E'* represent the electromotive forces of the inserted batteries, *w* the resistance of the conducting wire between A and B, *w'* that of the branch wire, together with the battery *E'*, *w''* the resistance

of the paper strip, i , i' , i'' , lastly, the strengths of the current in the resistances w , w' , w'' , then, according to Kirchhoff's form of Ohm's law, when the direction of the current is as shown by the arrows—

$$(1) w' i' + w i + w'' i = E + E'$$

$$(2) w' i' - w'' i'' = E'$$

$$(3) i' + i'' = i.$$

Hence follows for the desired case that i'' should be = 0—

$$E : E' = w + w'' : w'.$$

No current consequently passes through the paper strip when the resistances of the main and branch circuits are as the electromotive forces of the respective batteries. If in the same way station B inserts at the same time as station A the two batteries E and E', with the resistance w' properly adjusted, then the two cases have to be considered whether the batteries of the two stations reinforce or oppose one another. In the latter case the line ab is not traversed by any current, for the electromotive forces in A and B are equal and opposite. Derived circuits of the compensating batteries E' are, however, formed by the paper strips A and B. The former are consequently traversed by a current—

$$i'' = \frac{E'}{w' + w''}$$

A decomposition of the liquid with which the paper strips are saturated therefore takes place simultaneously at both stations, which, however, is not due to the currents passing by one another in the conducting wire, but are caused by local currents of the compensating batteries.*

* Mr. Zantedeschi, in two papers sent to the French Academy of Sciences on the 16th July and 6th August of last year, has laid claim to the credit of having already in the year 1829 proved the simultaneous passage of electric currents of opposite direction through the same conductor. His arrangement is very similar to Gintl's, and, like his, is opposed to Ohm's law. Though it does not appear proper to enter in these pages upon a special refutation of such hypotheses as have been called forth by no new phenomena not hitherto explained, it is still to be regretted that the setting up of such hypotheses is not directly denounced, as in many circles a great confusion of opinions has thereby arisen. That two equal batteries joined up in one circuit in opposition are actually inoperative is proved by the fact that no heat is developed in the circuit, for the production of heat is a necessary consequence of the passage of a current through a resistance, as well as by the further fact that no mechanical action takes place in the battery, without which a hydro-electric current is just as little conceivable.

When the batteries of the two stations are not opposed, but reinforce each other, the following equations hold good—

$$(1) w \cdot i + 2 w' i' = 2 (E + E')$$

$$(2) w' i' - w'' i'' = E'$$

$$(3) i' + i'' = i$$

$$(4) \frac{E}{w + w'} = \frac{E'}{w''}$$

whence follows, when i , i' and E are eliminated,

$$i'' = E' \frac{w + 2 w''}{w w' + 2 w' w'' + w w''}$$

In this case also there is a simultaneous decomposition in both paper strips, which is caused by the predominant current in the conductor.

To obtain a perfect telegraphic message the strength of the current passing through the paper strip must be equally strong both with simplex and duplex. Consequently in the case first considered we must have—

$$\frac{E + E'}{w + w' + w''} = \frac{E'}{w' + w''}$$

This equation is, however, only realised when w'' is put = 0. Consequently there can only be uniform and safe signalling when the resistance which the interpolated strip of paper opposes to the passage of the current is very small in comparison with the other resistances.

The same result is obtained in the case when the batteries are joined up in series, from the equation—

$$E' \frac{w + 2 w''}{w w' + 2 w' w'' + w w''} = \frac{E + E'}{w + w' + w''}$$

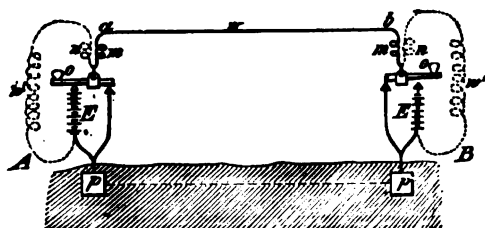
If the electrolytic arrangements are replaced by the coils of the two relay magnets, Gintl's arrangement of circuit can also be used for working duplex with electromagnetic telegraphs; but in this case the inconvenience of unequal currents with simplex and duplex working is even more disadvantageous than with electrochemical apparatus.

What specially militates against the practical utility of Gintl's duplex method as described is the difficulty incidental to the construction of double contacts, which have to work simultaneously and without interruption of the transmission. The electrochemical telegraph is generally only suitable for use on single unbranched lines, as it does not permit of translation, *i.e.*, the automatic signalling on of the message by the receiving instrument.

In the summer of 1854, Halske and I, and independently of us, Herr Frischen, telegraph engineer, of Hanover, took up the question of giving a practically serviceable form to duplex working with Morse instruments. We effected this in an entirely satisfactory manner, and certainly in an essentially similar way.

In the diagram, fig. 4, let $a\ b$ be the conductor connecting

Fig. 4.



stations A and B, m and n the two coils on the differentially wound relay magnet, o the key (contact lever) of the apparatus, E the battery, w' a variable resistance, p the earth-plates which form the connection with the earth. The connecting lines are the conducting wires. In the position of rest, *i.e.*, when neither of the levers o is pressed down, the line wire $a\ b$ is to earth at both stations, through one of the two coils m and the back contact of the lever o . If the lever o at the station A is pressed down, the connection of the coil m with earth is broken, and, on the contrary, it is made with the free pole of one of the batteries E , the other pole of which is to earth. The current of this battery now divides itself into two branches. One portion of the current traverses the coil m of station A, the line wire $a\ b$, the coil m of the station B, and passes to earth through the back contact of the key at that place. The other branch traverses the coil n of the station A, and returns through the resistance w' to the battery.

The coils m and n and the resistance w' must be so arranged that both the currents traversing m and n exert an equal and opposite magnetising effect upon the iron core of the relay magnet, and consequently no magnetism is produced in it. The current proceeding from one station will then only magnetise the relay magnet of the other station. With the arrangement represented in the diagram, it is sufficient to make the product of the strengths of the current of the two branch circuits into the number of windings of the coils m and n equal. As the strengths of the current in the two branch circuits are inversely proportional to their resistances, the number of windings of the two coils must consequently be as the total resistance of the respective circuits. If this ratio is obtained by proper selection of the resistance w' , no magnetism is produced in the magnet of its own relay; it consequently maintains its full susceptibility for the current coming from the other station.

For absolutely safe simultaneous speech, another condition must be added, viz., that the magnetising effect of the current coming from the other station must also remain of similar magnitude when the key is manipulated. If E denotes the electromotive force of the working battery, w the total resistance of the line wire $a b$, w' the compensating resistance, m and n the number of windings of similarly named coils, and if the resistance of the batteries is neglected as inconsiderable in comparison with the other resistances; there results from the foregoing the conditional equation—

$$\frac{m E}{w} = \frac{(m+n) E}{w+w'}$$

which equation is also satisfied when $\frac{m}{n}$ is made equal to $\frac{w}{w'}$, as is necessary for the equilibrium of the current starting from its own battery. From practical trials we have, as a rule, preferred making the number of convolutions of both coils, and consequently also the resistances of the line and compensation circuits, equal to one another, although by this means the consumption of covered copper wires for the relay and covered German silver wire for the construction of the compensation resistances is increased. We did this as greater resistances can be more easily balanced with sufficient precision for practical purposes, and the variable resist-

ances of the contacts are therefore of less importance ; but chiefly so as not (through too short a branch circuit) unnecessarily to weaken the battery current passing through the conductor, and make it irregular. As inconstant batteries, formed of carbon and zinc, filled with dilute sulphuric acid, are often used in telegraph offices, their electromotive force is very quickly diminished by polarization, when they are worked to any considerable extent. With short compensating circuits the incoming current will become specially variable when the conductor is incompletely insulated, and thereby the outgoing current considerably strengthened. Even when constant batteries are used, short derived circuits of the battery have the disadvantage that much larger cells must be employed, especially when numerous apparatus are to be worked by means of one battery.*

The problem of simultaneous speaking in opposite directions over the same wire can be conceived as perfectly solved by means of the construction described, as is confirmed by long practical experience. This method is, however, not applicable where the currents passing through telegraph wires are not of constant strength ; consequently neither with long submarine nor underground wires, nor in the cases in which a greater number of magnet coils are joined up in the circuit. In the first case the

* Dr. Stark, of Vienna, has described in the eighth number of the *Journal of the Austro-German Telegraph Association*, 1855, an improvement in our method of duplex working, which consists in modifying the apparatus sent by us to Vienna by making the ratio of the number of windings of both branch circuits unequal. I have already stated the grounds that have generally induced us to make the resistance and the number of windings of both branch circuits equal. Dr. Stark calculates that his magnet, provided with an unequal number of windings, has led to an increased sensitiveness in the proportion of 1 to 1.67. In this matter, however, he has neither taken into consideration that the resistance of the windings through which the current passes, and consequently that of the whole conductor, is increased by its adoption, nor that for a greater resistance a more suitable proportion of the diameter of the wire can be chosen, and consequently the advantage calculated from it of the unequal number of windings can be closely compensated. An increase of the resistance of the magnet coils interpolated in the circuit is not, however, advisable, as the leakages of the leading wires due to imperfect insulation act all the worse the greater the resistances existing between them and the battery. We have tried our first experiments in the direction of Dr. Stark's improvement, and have since produced many duplex apparatus with small compensation resistances, but found practically that the balance of the currents is most easily restored and maintained when both wires were wound on together and with the same number of convolutions. In this way the advantage is specially gained that the wires of a differential galvanoscope can be joined up in both branch circuits, and with its help the insertion of the proper compensation resistance can be easily effected.

principal coil predominates at the commencement of the current ; in the second the compensation coil. Consequently in both cases no complete balance can be got.

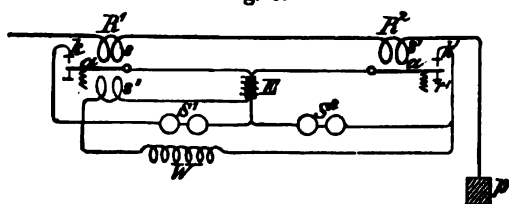
Halske and I have obtained a less favourable practical result by the solution of another problem—that of simultaneously signalling with two apparatus in the same direction by means of Morse printing instruments.

If by means of suitable mechanism two batteries of different strength are joined up to one end of a telegraph line and to earth, so that one or other of the batteries or both together can be interpolated without breaking the continuity of the circuit, three different strengths of current can be produced in the conductor. If battery II is twice as powerful as battery I, then the current strengths produced by I, II and $I + II$ are in the proportions of 1, 2 and 3. If now two relays are connected up at the other end of the conductor between it and the earth, of which the first is worked by current strength 1, while the second is brought into action by current strength 2, then the solution of the problem requires that relay I should be set in motion only by current strengths 1 and 3, but not by current strength 2. This can be effected in many ways. We first sought at the commencement of last year to compensate current strength 2 in relay I by means of a local battery. This was done by winding the magnet of relay I with two wires, of which one was connected up in the main circuit, whilst the other was traversed by a branch current from the local battery when relay II had attracted its armature. This local current was so regulated by a rheostat that it produced in relay I an equal and opposite magnetism to that of current 2. Hence when battery II was put in circuit, relay I was momentarily set in motion. As soon, however, as relay II had also attracted its armature the working of the compensation current began, and the armature of relay I fell away again before the momentary closing of the local circuit produced by it could produce a mark on the paper strip. If, however, battery I was also put in circuit, current strength 3 circulated in the conductor, the balance of the currents in relay I was thereby disturbed, and it attracted its armature through the action of the difference of the currents, *i.e.*, by current strength 1. The result of the experiment was unfavourable, as could easily be anticipated. Setting aside the difficulty of main-

taining two currents from different batteries in constant balance, regular writing was not once obtained in the room, principally because the working of relay I was too sluggish when the compensating coil is closed by the local battery, and because relay II does not work safely alternately with current strength 1 and 3, as should have been the case.

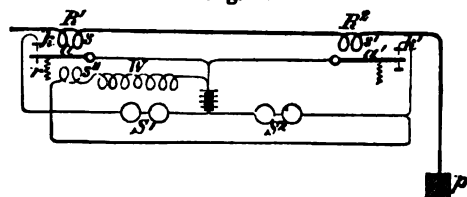
The diagram for the solution of the problem of duplex working is represented in fig. 5. The coils of the relays R^1 and R^2 are

Fig. 5.



denoted by s and s' , the compensating coil of the relay R^1 by s'' ; a and a' are the armatures of the two relays, k and k' their contacts by the contact of which with a and a' the circuit of the local battery is closed through the coils S^1 and S^2 of the writing magnet. Further the contact $a'-k'$ establishes a shunt circuit from the

Fig. 6.



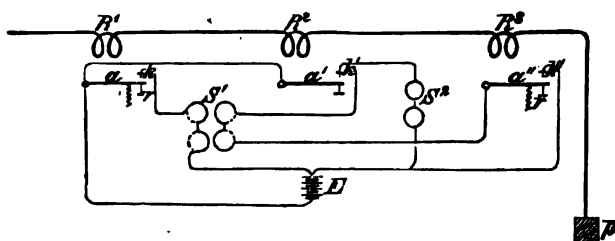
battery E through the rheostat w and the second coil s'' of relay 1. The rheostat w is so adjusted that the coils s and s'' exercise with current strength 2, equal and opposite magnetising effects on the iron core of the magnet, and are consequently balanced with this current strength.

In the diagram fig. 6, coil s'' is traversed by a continuous current from battery B , and in the same direction as coil s . If current strength 1 passes through the conductor, then the armature is

attracted by the joint action of both coils. If, however, the armature α' is also attracted by current strength 2, then the local current through s'' ceases and the armature α falls away. Current strength 3 on the other hand attracts it.

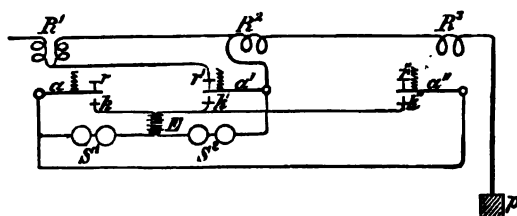
With the help of a third relay R^3 , which attracts its armature with current strength 3, the uncertain neutralization of current strength 2 in relay R^1 by a local current can be done away with. Fig. 7 and fig. 8 represent two diagrams of this kind. The letters

Fig. 7.



have the same meanings as above given. Remembering that the thick lines represent the line current, and the thin the local currents, the direction of the currents will be understood without special description. It is only necessary to remark that in diagram fig. 7 the action of current strength 2 is compensated in the

Fig. 8.



double wound magnet of the writing apparatus, whilst in diagram fig. 8 this compensation takes place in the relay R^1 through the line current itself, since the current itself is forced to traverse the two similar coils of the relay R^1 in opposite directions, when the armature α' is attracted, and hence its contact with its rest contact r' is broken.

A number of similar current circuits can be formed with little trouble, by which the problem of duplex speaking may be solved with more or less success. We have, however, not yet succeeded in gaining a practically serviceable result by any of these methods. This can be easily explained, since with duplex speaking three different current strengths must be made use of and regulated in order to keep the telegraph signals of both apparatus distinct, whilst with duplex speaking only two current strengths are requisite. Duplex speaking appears, therefore, to have but little prospect of further development.*

For the sake of completeness, I will now mention an experiment which Halske and I made to obtain simultaneous multiple uses of a wire in quite a different manner.

When rapidly alternating currents of equal strength and duration such as are produced in the coil of an iron armature, which rotates before the poles of a powerful magnet, are allowed to traverse the coil of an electromagnet, no magnetism is produced in its iron core. An electrodynamic relay simultaneously traversed by these currents (for instance, a Weber's electro-dynamometer with contact arrangement) is, however, set in action by them. A weak constant current, which is allowed to pass either alone or simultaneously with the alternating currents through the same coils, causes on the contrary the electromagnet to work, whilst

* Dr. Stark, in No. 10, Year II. of the Journal of the Austro-German Telegraph Association, has set forth two schemes for duplex working, of which one closely agrees with the first above described. The other, with three relays, is at least not more suitable to the purpose than the one attempted by us. Although we have hitherto refrained from publishing our experiments, as we wished first of all to bring them to a successful issue, we have in the course of last year made frequent verbal and written communications to all those interested in the subject. In August of last year I communicated to Professor Pouillet, of Paris, amongst others, some diagrams of connections for reproduction in a work that was being printed.

At the end of his paper Dr. Stark makes an erroneous statement, which ought not to remain unnoticed, as it proves that he also shares Dr. Gintl's opinion that electric currents, as it were, pass through one another without mutual disturbance. He maintains, namely, that duplex may be united with the duplex speaking described by him; that, consequently, one can telegraph simultaneously with four instruments over the same line. Duplex, as well as duplex, speaking over the same wire, and with Morse writers or similar instruments generally, which require currents of variable duration for the reproduction of their signals, is only possible by altering the strength of the currents in the line wires. Duplex and duplex speaking in the manner above described must therefore necessarily mutually hinder one another, and are consequently not simultaneously practicable.

the electrodynamic relay which requires stronger currents is not affected by it. In this manner, therefore, when the oscillating currents have sufficient strength, diplex speaking can be obtained with safety. As the above described duplex process can be applied as well to alternating as to continuous currents, we have here the possibility of employing diplex and duplex speaking at the same time.

This method is, nevertheless, just as little applicable for practical use. The use of such strong currents as an electrodynamic relay requires is in general inadvisable. Such rapidly alternating currents as are necessary in order that the electromagnetic relay may remain quite inactive are, moreover, not serviceable, because they cannot be conveyed to great distances. For underground and submarine cables this phenomenon requires hardly any further proof. The electrostatic charge first described by me in these pages, and more lately confirmed on various sides, and especially by the experiments of Faraday, quite destroys short alternating currents. If the alternations are much quicker than the times for charging the whole wire, then positive and negative charges will move on in the wires one after the other, and in passing forwards must flow into one another, and therefore very quickly lose their intensity. As I shall show later on in another paper respecting the phenomena of charge, overhead conductors may be considered as great Leyden jars, although of much less capacity than underground conductors of the same dimensions, in which the air found between the wire and the earth takes the place of the glass of the jar. As well on account of the charging of overhead wires thus brought about, as of their imperfect insulation and connected therewith, the polarization of the wire altering with the direction of the current and of the plates making connection with the earth, there is brought about a quickly increasing diminution of the current with the distance of the alternating current from the source.

ANSWER TO EDLUND'S REMARKS ON THE SENDING
OF SIMULTANEOUS MESSAGES.*

MR. EDLUND, in the eighth number of this Journal, remarks, in the first place, that the method of duplex working by means of branched currents described by me entirely agrees with what he made use of in the year 1848 for the measurement of the Faraday extra current, and he argues that the arrangement of circuits then used by him could with little alteration be used for duplex working. Mr. Edlund could with equal propriety have gone still further. Two of Becquerel's differential galvanometers with double windings, such as have been generally employed for a long time past for the measurement of resistances, constitute a perfectly serviceable apparatus for working duplex with branched circuits. It was only necessary to comprehend this idea, and make it practically serviceable. But, as is known, the road to discovery is very seldom the direct way to the goal, which as a rule lies near, as Mr. Edlund himself again very convincingly proves.

Mr. Edlund further shows that he introduced duplex working in August, 1854, on a Swedish line, and described his method in the number for June, 1855, of the Stockholm Academy of Sciences. As this paper has, however, not been published to my knowledge in a more universally accessible language, it has remained quite unknown to me. I certainly know that Mr. Edlund had invented a duplex system and had taken patents for it in various countries. I was also present, as is quite correctly stated, at some successful trials in Paris with a relay constructed according to his designs. I could not, however, find out how it was made, and had to assume that the construction was to remain a secret. The duplex method invented by Mr. Frischen on the one hand, and by Halske and me on the other, by means of branched currents is, on the contrary, fully described in several German periodicals, and amongst others in the work of L. Galle † on the Electric Telegraph which appeared in Leipsic in December 1854, and was consequently published six months previously to Mr. Edlund's paper.

* Poggendorff's *Annalen d. Phys. u. Chem.*, 1856, p. 310.

† *Catechism of the Electric Telegraph*, by L. Galle, Leipsic, 1855.

As, according to prevailing custom, the date of publication and not that of experiments secretly carried on decides priority, it matters little whether Mr. Edlund or we made the earlier experiments in this direction. We, as well as Mr. Frischen, were at a disadvantage in this matter, as chemical telegraph lines are unfortunately inaccessible to us; we were, therefore, obliged to complete our experiments in a room, so as not to be too often obliged to seek the acknowledged kindness of the telegraph direction of neighbouring lines. Nevertheless, Mr. Frischen made the first successful experiments on the line, as he is prepared to show if Mr. Edlund should so desire.

Mr. Edlund discovers an essential difference and a superiority in his arrangement in giving the compensation branch a less resistance, and a correspondingly smaller number of windings than the line branch. At first we always did this, as I have clearly enough shown in my paper, and later very often, after we had provided those apparatus which were to work on long lines with similar coils in accordance with Mr. Frischen's experience. Mr. Edlund is, indeed, quite right when he remarks that the magnetic balance in the relay of the sending station is disturbed, whilst the key of the receiving station passes from one position of rest to the other. In his calculation, however, he overlooks the fact that in practice we have not to do with perfectly insulated lines as he assumes them to be. The greater, however, the leaks on the conductor employed, the less completely will its influence on the variations of resistance at its end depend on the strength of the battery current. Nevertheless, Mr. Edlund would be right in reducing this constantly disadvantageous influence as much as possible if other considerations were not opposed to it. These are, that both in consequence of imperfect insulation of the line and of the electrostatic charge of the wire preceding the steady development of the galvanic current, the current traversing the branch wire of its own relay becomes much stronger than that portion of it which reaches the windings of the distant relay, and that the first named much stronger current must be at every moment in equilibrium with its branch current. As now in a large coil of thin wire, the development of the current on making contact becomes considerably reduced in speed by the induced

current, as Mr. Helmholtz * has shown by measurements, whilst it occurs instantaneously in the compensation coil consisting of a few windings, it is clear that in this respect equally long branch wires wound together have an advantage over Mr. Edlund's proposed unequal coils. That the retardation of the commencement of the current has no inconsiderable effect follows from the fact that duplex working hardly ever succeeds with the previously known methods, when the magnet coils of any intermediate stations are interpolated in the line. This, then, has hitherto been the most essential hindrance to the general use of duplex telegraphy.

I have no wish to deny hereby in any way that a diminution of the resistance of the compensating branch is applicable in the majority of cases ; I only assert that this question is too complicated to be decided otherwise than by practical experiment. We, therefore, did not hesitate to depart from our previous practice, and to adopt the idea of equality of both coils as indicated by Mr. Frischen on account of his large experience in the use of duplex instruments on long lines, as the object was to establish a common uniform construction.

The calculation by which Mr. Edlund proves that the increase of the power of the battery, which becomes necessary owing to the reduction in the resistance of the compensating circuit, does not essentially enter into the question, as it is based on the assumption of completely insulated conductors. Practical constructions must, however, be based upon what is most unfavourable. If duplex telegraphy is to be brought into general use, it must be made safely practicable, when only a small percentage of the current sent into the line reaches the further end. As, however, as a rule batteries of very low resistance were employed, the temporary objection raised by me against the reduction of resistance of the compensation circuit is indeed rather unimportant.

Finally, Mr. Edlund attacks my statement that the strengths of the current in the line vary with the amount of the polarization of the inconstant battery employed, and seeks to show by a calculation which is incomprehensible to me, that the reduction of the line current brought about by the polarization of the battery is

* Pogg. Ann. LXXXIII. p. 505.

independent of the amount of such polarization. As, however, the polarization affects the calculation as a reduction of the electromotive force of the current, therefore, self-evidently, the strengths of the current in all the branches of the circuit must be reduced proportionally to the amount of polarization. If then, in one case, the amount of polarization is, according to Mr. Edlund, equal to p , and in the other to np , the strengths of the current diminish in all the branch circuits of the battery—consequently also in the relay of the distant station—from the commencement of the current up to the moment in which the electromotive force of the battery is diminished respectively by the amount p and np in the proportion of these reductions. If, therefore, the reduction of the incoming current is in the one case equal to p , and in the other case equal to np , it is not, therefore, independent of the amount of the polarization as Mr. Edlund asserts, but directly dependent on it.

The arrangement, described by Mr. Edlund in his remarks, for regulating the balance of both coils by varying the number of windings which belong to one or other coil, is very ingenious, and, notwithstanding the great complication of the construction, can frequently be used with advantage.

Finally, I wish to take advantage of the present opportunity to rectify an erroneous statement I have made in a remark in my paper. I objected to an assertion of Mr. Stark referring to another matter, regarding the opinion that it was not possible to use the same wire at the same time for duplex and duplex speaking, as both were based on the variation of the strength of the current in the line. This is quite correct, but not the deduction from it. As the three batteries of the duplex sending station combine their currents with that of the other, there arises a sufficient number of currents of different strength to keep the signs of the four apparatus distinct. Naturally, there can never be any question of the practical application of the theoretical possibility of simultaneous duplex and duplex speaking.

CORRECTION OF THE FINAL REPLY OF MR.
EDLUND ON THE DUPLEX TELEGRAPH.*

NOTWITHSTANDING my answer to his remarks, Mr. Edlund maintains three of his charges, and hence compels me, and more especially on account of the summary tone of his final reply, to make a short, but I hope, intelligible correction.

Although I have conceded to Mr. Edlund, that compensation coils of less resistance than that of the line are more advantageous in many respects, and although I have over and over again stated that Halske and I have only lately departed on account of comparative experiments from our original construction, in which we employed compensation currents of greater strength, Mr. Edlund again takes the trouble to repeat his preference for them. I stated in my answer that the extra current of the magnet coils, the induced currents produced on long overhead conductors, and the always more or less imperfect insulation of the wires must be taken into consideration in the theoretical comparisons of both constructions, and showed that the question was too complicated to be settled in any other way than by comparative experiments. Mr. Edlund takes notice in his final rejoinder only of the objection of imperfect insulation of the conductors, certainly allows that it in part compensated the disadvantage of large compensation coils, but asserts that it was only slight in the lines in question, and informs me that the phenomena of the galvanic current are sufficiently known to admit of previous calculation of the actions of different coils.

As Mr. Edlund has only taken notice of one of the arguments brought forward by me, and as it is precisely the imperfect insulation of the conductors, and the consequent passage of the current from one conductor to the other, together with the disadvantageous influence of the extra current that have hindered the general use of the duplex and diplex methods, the question whether a greater or less compensating resistance is more advantageous has at present no further practical interest, and it is not worth while to make more searching calculations on the subject.

* Poggendorff's *Annalen der Phys. u. Chem.*, 1857, p. 653.

Mr. Edlund agrees that the battery power must be increased with compensating coils of low resistance to make the line current as strong as in the other case. He calls this increase insignificant. I asserted that it becomes more important when the insulation of the conductor is bad. Mr. Edlund says it is impossible for him to conceive what I meant to say by this. If half the current is lost through derived circuits, the battery must be double as powerful, in order that the incoming current may have sufficient strength. Hence, if the battery has to be increased on well insulated conductors, say by 10 cells, when compensating wires of low resistance are employed, it must be increased by 20 when half the current is lost. As now an increased employment of 20 cells is of greater consequence than one of 10 cells, the correctness of my remarks cannot be called in question. Besides, the number 20 is further increased, when account is taken of the resistance of the additional cells.

Finally, Mr. Edlund advises me to read again through his calculation, which is to prove that the reduction of the line current by polarization of the battery would not be increased by strengthening the battery current, and hopes that I shall then understand it.

I had made use of the expression that it was incomprehensible to me, because I thought that Mr. Edlund had only overlooked the mistake which had occurred in it, and that it would be sufficient to draw his attention to it. As, however, he again maintains the correctness of his calculation, and asserts that mine only proves his assumption, I can no longer avoid a closer consideration of the very simple physical fact in question.

The current of a variable battery divides itself into two branches, of which one traverses the line, the other a branch wire. This branch wire has in one of the two cases considered the same resistance as the line, in the other a considerably lower resistance. The battery is, in the second case, increased to such an extent, that the original line current is as great as in the first case. Now Mr. Edlund wishes to show, by his calculation, that the diminution of the line current through the polarization of the battery is exactly the same in both cases, although the polarization is proportional to the current of the battery, consequently in the second

case is much greater than in the first. He says in his remarks, vol. viii., p. 636 :

"If the resistance of the derived circuit is equal to that of the whole line, half of the current passes on to the next station. We can consequently represent the whole current by 2, and each of its two portions by 1. If the polarization of the battery is represented by $2p$, a reduction will take place in the current passing to the further station equal to p etc."

In this "equal to p " lies the error of the calculation. The diminution is not equal to p , but proportional to $2p$. If then in the second case the polarization is equal to $2np$, as Mr. Edlund assumes, the diminutions of the current are in both cases as $2p : 2np$, consequently as $1 : n$, or as the current strengths of the batteries.

As Mr. Edlund is not convinced of the incorrectness of his calculation, from the calculation already given by me in my remarks, I will explain the desired proportion of the current strengths in a more general form.

Let s and s' be the strengths of the sent and received currents in the one case, and s and s'' in the second case, then $\frac{s-s'}{s-s''}$ is the desired proportion of diminution of current. If now the battery consists in the first case of n , and in the second case of m cells, of which each has the electromotive force e ; if further the resistances of the line of the branch wire and of the battery are represented in the one case by l, w and W , and in the second case by w, w' , and W' , and further, the polarization of a cell of each battery by p and p' , then—

$$s = ne \frac{w}{lw + lW + wW} = neW, \text{ and}$$

$$s = me \frac{w'}{lw' + lW' + wW'} = meW'$$

whence $nW = mW'$. Further :

$$s' = (ne - np)W \quad s'' = (me + mp_1)W', \text{ and}$$

$$\frac{s-s'}{s-s''} = \frac{npW}{m p' W'} = \frac{p}{p'}.$$

Hence the reductions of strength of the currents are as the

polarization of equal number of cells of the batteries employed, or as the final strengths of the current of the batteries.

Perhaps Mr. Edlund has not intended to represent by $2p$ and $2np$ the polarizations themselves, but the opposed currents which may be substituted for them, and which will produce equal diminutions of current, without considering that they make their appearance in circuits of different resistance, and are therefore inversely as their squares.

ON ELECTROSTATIC INDUCTION AND RETARDATION OF THE CURRENT IN CORES.*

SEVERAL years ago I described in this Journal, Vol. LXXIX., 1850, p. 481—*Annales de chim. et de phys.* 3^me Sér. t. XXIX. p. 385, and elsewhere, the phenomenon of a strong current of slight duration occurring when a well-insulated telegraph wire is joined up to the free pole of a galvanic battery, the other pole of which is to earth. I also showed that this phenomenon of the influence action of voltaic electricity in the wire is to be ascribed to the dampness of the earth on the outer coating of the wire, acting as an outer coating to the core, and must also occur when one end of the wire is put to earth. The diagrams of charges which accompanied my paper, published at that time in this Journal, gave a complete explanation of the relative amount of electricity which existed statically at each point of the outer covering of insulated and of earthed underground wires or cores, when the thickness of the wire and of the insulated covering remained unchanged.

I was prevented at that time by other occupations, and later by being engaged in replacing the former underground telegraph wires by overhead lines, from further following up the experiments on this subject and supplying the deficiencies I had mentioned. But since the method proposed by me in the year 1847, of insulating telegraph conductors by covering them under pressure

* Poggendorff's Ann. d. Phys. u. Chem., 1857, Vol. CII. p. 66.

with gutta-percha, has been taken up in England with better material than was at our disposal at that time, and has been frequently employed for underground and especially for submarine cables, general attention has been directed to the electrostatic charge of these wires, and to the consequent retardation of the electric current at the further end of the line. Without knowing of my communication, celebrated English physicists and mathematicians, especially Faraday, Wheatstone, and Thomson, have made the electrostatic charge and the retardation of the current in cores the subject of their study, and have published very valuable memoirs thereon, by which partly my earlier observations have been fully confirmed and extended, and partly, especially by the calculations made by Thomson, the gaps left by me have been filled up.

I shall frequently refer to these new labours in the second part of my paper. Previous to their appearance, I was already engaged upon experiments on electrostatic induction due to voltaic electricity, which, as explained, were often stopped owing to other engagements. I was led to examine them because of phenomena which I was not able to bring into agreement with the influence theory formerly existing. The labours of the English physicists above referred to further strengthened me in my design, as they proceeded (especially Thomson in his calculations) entirely upon the theory set up by Faraday of molecular induction as the cause of electrical influence, without giving further proof of its correctness than formerly existed. My object was simply to discover and confirm by purely experimental means the laws of electrostatic induction by voltaic electricity, so as thus to obtain a safe basis for practical constructions. I certainly could not avoid scientific considerations and side issues about the theories which are founded on experiments with electricity of high tension, but I only place an important value on them in so far as they render clearer the results obtained for voltaic electricity.

Volta has already shown by his condenser experiments, that bodies brought into contact with the insulated pole of a galvanic battery act on neighbouring conductors. Guillemin * first published experiments, from which it followed, that the charge of a jar charged by voltaic electricity, when passed through a galvano-

* *Compt. rend.* Vol. XXIX., p. 632; *Ann.* Vol. LXXIX., p. 335.

meter, was able sensibly to deflect the needle of the instrument. I had certainly already observed in the summer of 1849 that the strong deflection of the needle of a galvanometer which was joined up between an insulated core lying in damp earth or water, and a galvanic battery with one pole to earth was due to an electrostatic charge of the core, and this opinion, as well as the experiments on which it was founded, was communicated to the Berlin Academy of Sciences on the 18th January, 1850; my paper was, however, printed later than that of Guillemin. He joined the tinfoil coatings of a condenser made of thin silk or gutta-percha of one to two square metres surface alternately in quick succession with the poles of an insulated galvanic battery and the wires of a galvanometer. This was effected by means of a disc commutator, which was set in rapid rotation. He found in this way that the needle of the galvanometer was deflected, and that the amount of this deflection increased with the velocity of rotation of the commutator and the strength of the battery. Experimental measurements were not made to my knowledge, either by him or others, before this. For the experiments made in the year 1849, I made use of existing underground telegraph conductors. These were insulated at the further end, and the near end joined up alternately by means of a commutator to the free pole of a galvanic battery, the other pole of which was to earth and to an earth wire. The wire of the galvanometer inserted between the underground conductor and the commutator was consequently alternately traversed by charge and discharge currents. If the time of charge was small as compared with the time of oscillation of the needle, the sine of half the angle of deflection could be taken as a measure of the quantity of electricity which had passed through the galvanometer. It was consequently also the measure of the amount of charge. I obtained in this way very accurate results with conductors which were not too long, and very well insulated. It appeared that the deflection for the charge was as great as that for the discharge. The charge was further proportional to the length of the conductor, and to the electromotive force of the battery used.

For more exact measurements this method was not available, both on account of local difficulties, and because the long conductors were not sufficiently well insulated, and the charging currents lasted too long.

Fig. 9.

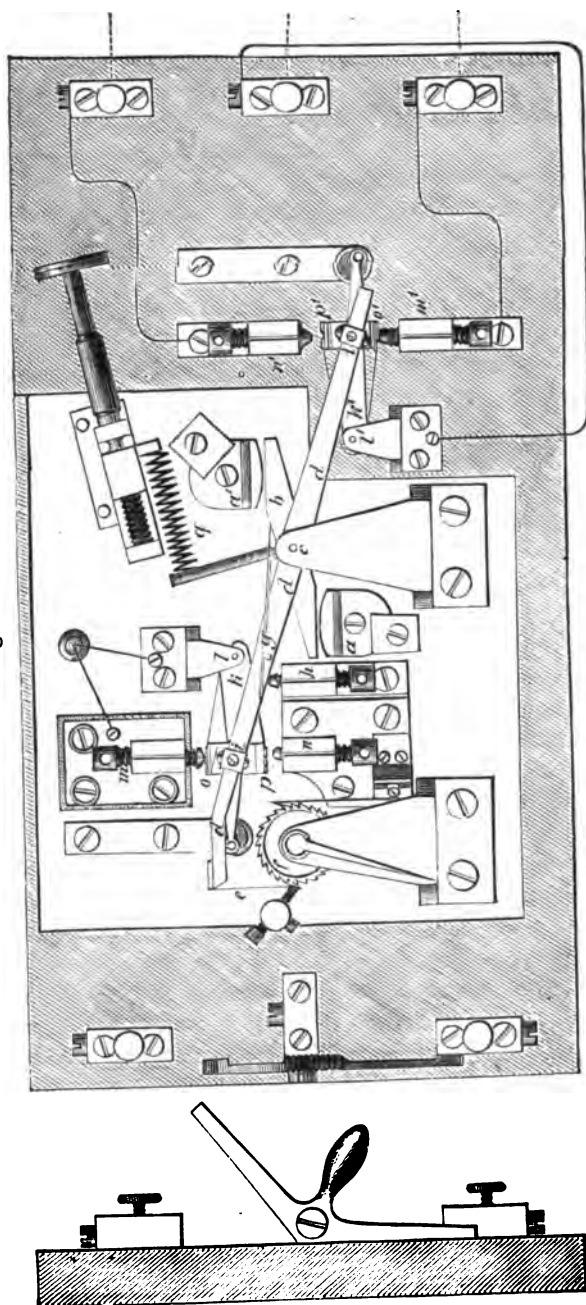
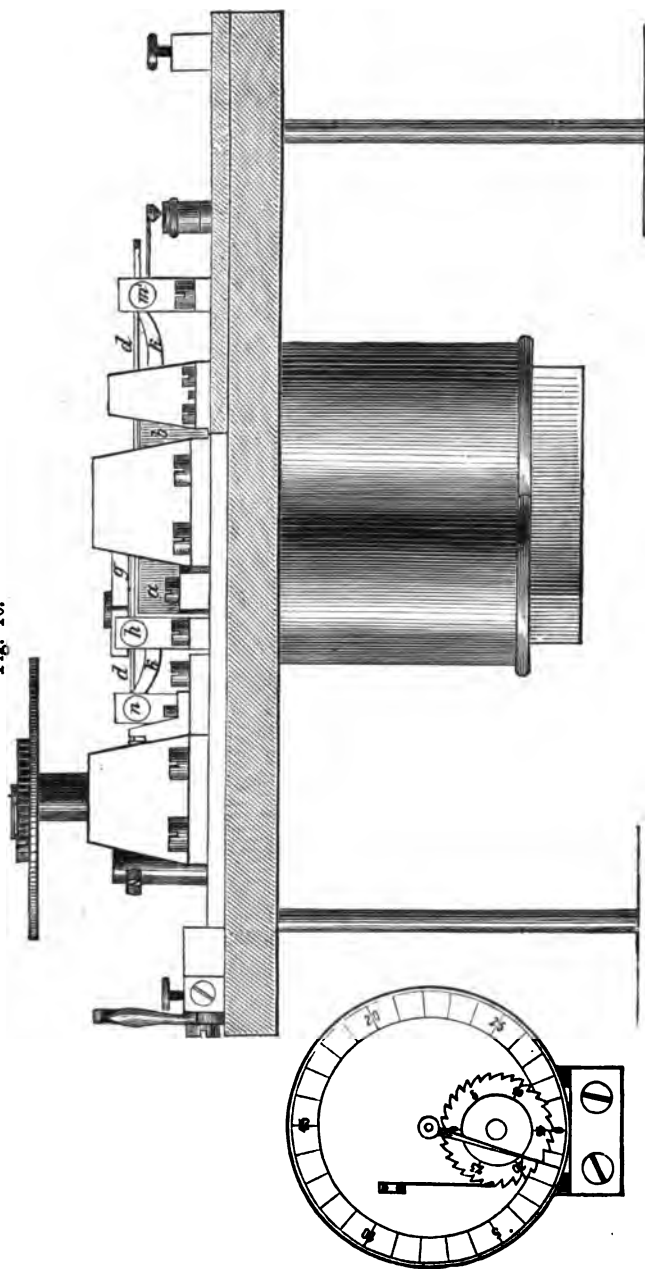


Fig. 10.



My later experiments were in the first instance only directed to determine the relative magnitude of the charge of cores, having different sizes of wire and insulating covering, as well as the charge between twin wires, which lie at a small distance from one another, in the interior of a common covering of gutta-percha of circular or elliptic section. With this object I had several wires of a mile in length prepared, the insulating gutta-percha covering of which varied in thickness within practically applicable limits. These wires were wound on wooden drums, and laid in a wooden trough lined with zinc, which was filled with water. If now a galvanic battery of 20 to 60 Daniell cells, and a single-needle galvanometer with 24,000 convolutions, are inserted between such an insulated wire and the zinc of the trough, I obtained deflections of the needle sufficiently large for measuring the charge.

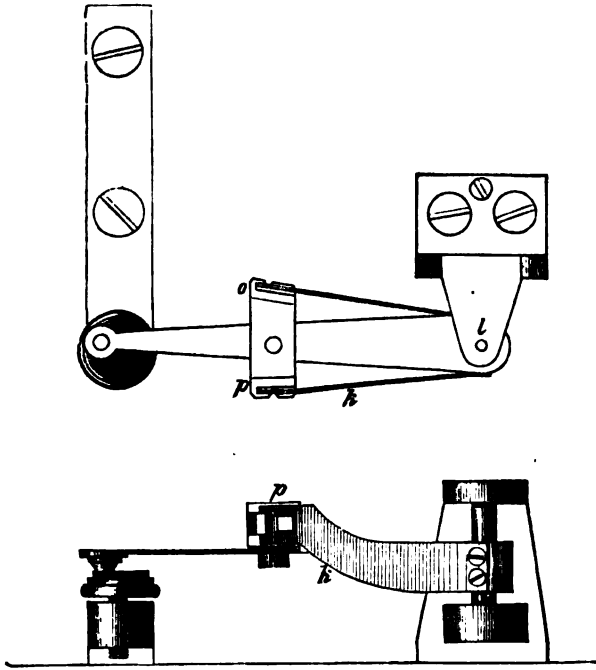
I soon convinced myself, however, that I could obtain no safe and generally satisfactory results in this way. The least imperfection in the insulation had a very important influence on the amount of the deflection of the needle, which could not be accounted for by the variability of the current passing through the gutta-percha. The duration of the charging current was besides large enough to exercise an influence on the deflection of the needle. Finally, it happened that the results of the measurements were so decidedly different from what was to be expected from theory, that a more general enquiry of the occurrence of electrostatic induction with voltaic electricity was requisite.

In this enquiry I have employed the method used by Guillemin, passing a continual series of charge and discharge currents through a sensitive galvanometer. In order to adapt this method to exact measurement, a commutator had to be made, which would effect the commutation unfailingly and with absolutely constant velocity. I made use for this purpose of the same mechanism that I used for my dial telegraph with automatic current break, which I have frequently described. I was led to this by the observation made with this telegraph that the speed of the instrument was only very slightly affected by the strength of the current. This is explained by the circumstance that with greater strength of current the attraction of the armature is certainly effected more quickly; its return, however, is so retarded by the stronger remanent mag-

netism that the time of the whole oscillation remains almost unchanged. Figs. 9 and 10 show the automatic commutator in plan and elevation half size.

The piece of iron *b* serving as armature oscillates between the poles *a* and *a'* of the electromagnet placed under the base of the apparatus. It rotates about the vertical axis *c*. The horizontal arm *d* is fixed to the axis, and moves the toothed

Fig. 11.



wheel *f* through the spring pawl *e*, and shortly before its oscillations are checked by the stop-screws *m* and *n*, moves the slides *k* and *k'* by means of the contact pieces *i* and *i'*, provided with insulating stones. These slides rotate about the axes *l* and *l'*. Their motion is limited by the contact screws *m* and *n* and *m'* and *n'* respectively. The spring end of the slides *k* and *k'* is provided with a rounded point of glass-hard steel, which on each motion of the slide must pass from one contact to the other over

the edge of a flat stone prism. In Fig. 11 this mechanism is shown by itself. The force with which the point tries to slip down the inclined surface of the prism, keeps the slide in firm contact with the contact screws, limiting its motion. As already mentioned, the jewels of the metal pieces fastened to the lever d strike upon the flanges o and p and $o' p'$, fixed for this purpose to the slides, just before the completion of each oscillation, and consequently when the velocity of the lever is at its greatest. The time which the slides require to travel their short path is, therefore, exceedingly small. It is still necessary to mention the use of the spring g fixed to the lever d , and the screw h standing opposite to it. They serve to accelerate the oscillations. The screw is so fixed that the spring strikes it, when the armature following the attraction of the magnet pole has travelled over about two-thirds of its course. The spring must consequently bend, and so increases the strength of the spiral spring q opposing the attraction of the magnet pole. It would be more accurate to employ instead of the two springs g and q , a single spring so short that its strength augments proportionately to the attraction of the magnet, which, however, cannot be carried out. The wheel f is provided with 60 teeth, the number of its revolutions per minute gives therefore the number of double oscillations per second. The slide k and the contact screw m form part of the circuit of the battery, which sets the apparatus in motion. It consists of from three to four Daniell cells. If the springs g and q are rightly adjusted, the oscillations follow one another very quickly and quite uniformly, as is shown from the following series of experiments. The slide k as well as its contact pins, are well insulated with ebonite. They form together a simple, quick, uniformly working commutator.

For testing the apparatus, I set it to work with three Daniell cells. This is carried into effect by means of a key exactly at the moment that a magneto electric clock in the room is on the stroke of a minute. On the stroke of the second minute, the key is again opened, and by means of a counter and index the number of oscillations made is read off. The apparatus is then set in motion again, and each time after an interval of an hour the experiment is repeated. I obtained the following figures in this way:—

Time.	Number of Oscillations	
	In 2 Minutes.	Per Second.
0		
1	7231	60·25
2	7250	60·41
3	7215	60·12
4	7211	60·08

The small differences can be sufficiently explained by the difficulty of making and breaking contact exactly at the moment the clock is heard, by variations of the current, and by the not quite constant work the magnetism must perform to move the wheel and counter. In my later measurement I therefore omitted both, and satisfied myself of the uniformity of the motion of the commutator by a method to be described later on.

The galvanometer employed is a carefully constructed sine galvanometer, with prismatic telescope of treble power, by which I could set the needle with great accuracy to one-tenth of a division from the zero line; with a vernier divided into five parts I could read $\frac{1}{5}^{\circ}$ and estimate $\frac{1}{10}^{\circ}$. The galvanometer is wound with two wires, the ends of which are brought to insulated terminals, so that I could connect them up in parallel, singly or in series. The galvanometer was well insulated with blocks of ebonite, carefully placed horizontally, and the suspension of the thread brought into the axis of rotation. To get rid of any slight eccentricity in the divisions, I used as a rule for each experiment two deflections with the direction of the current reversed, and took the mean. When using astatic needles, to which I always allowed sufficient directive force, the zero point was re-determined after each reading. The current was reversed by a commutator placed near the instrument. By means of a second commutator placed near the battery, the connections could be quickly changed, so that any current through the insulating material passed directly through the galvanometer. The batteries I used were exclusively Daniell cells. They were recharged every second day, and then remained sufficiently constant throughout the day. I always carried out comparative observations immediately after one another. At the commencement and end of each series of observations, I

noted the deflection of the needle by the continuous discharge of a unit jar. If it was not unaltered the intermediate observations were repeated. This served to prove that no alteration had taken place in the instruments and batteries. This process also gave a means of comparing the observations made at different times, of

Fig. 12.

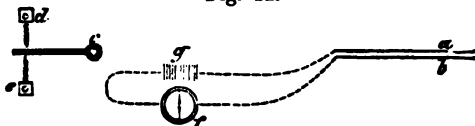


Fig. 13.

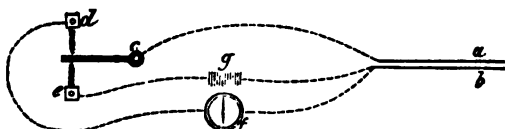


Fig. 14.

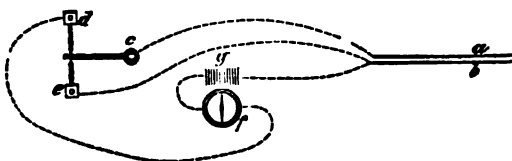
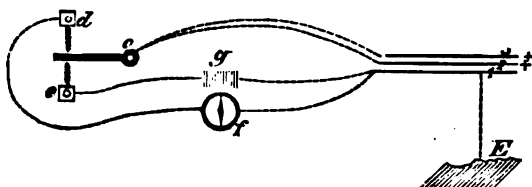


Fig. 15.

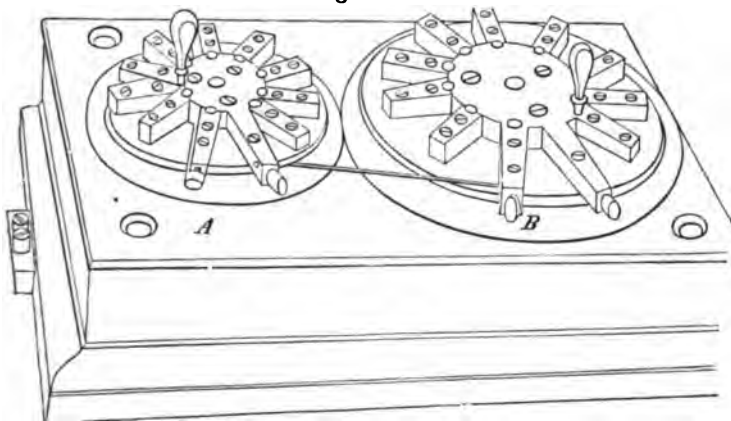


which, however, I but seldom made use. In the following tables of observations, the numbers in the first column headed n signify throughout the number of cells used. In the first table the readings of the vernier of the divided circle are given. In the later ones, only the half-difference of both readings is given. The sine of this angle is proportional to the quantity of electricity which has

passed through the galvanometer in the unit of time, and consequently with a constant action of the electromagnetic commutator is also proportional to the amount of each separate charge or discharge, it being assumed that the magnetization of the needle undergoes no variation.

The connections used for these measurements are represented in Figs. 12, 13, and 14 : *a* and *b* are the coatings of the jar or condenser, the charge of which is to be tested, *c* is the oscillating tongue of the commutator, *d* and *e* its insulated contact stops, *f* the galvanometer, *g* the battery. If the commutator was arranged as in diagram 1, the galvanometer was joined up with the battery

Fig. 16.



directly between the condenser plates or the coatings of the jar, and the least imperfection in the insulation would show. The position 2 of the commutator conducts all charges, position 3 all discharges through the galvanometer. As the first series of experiments proved the exact equality of the charge and discharge currents when the insulation was perfect, I made use later of discharge diagram 3 only.

The condenser employed for the following experiments consisted of a piece of mica 0.98 of a square decimetre and 0.1^{mm} thick, which was covered with tinfoil on both sides, so that 5^{mm} width of the edge remained uncovered. The condenser was placed on an insulated metal plate, which was in connection with the tongue of the commutator. The upper coating was connected by means of an

insulated wire with the battery and galvanometer, so that the point of contact could be displaced at pleasure. Further, I could connect a wire leading to the gas-pipe at pleasure with one or other coating. A rheostat was joined up in the wire leading from the coating *a* to the commutator *c*. This is specially shown in Fig. 16. It consists of a thin German-silver wire covered with silk, which is wound on two rollers A and B. After a resistance of one German mile, expressed in terms of an iron wire two English lines in diameter, had been wound on the small coil A, or a resistance of ten German miles on the large coil B, a branch wire was attached to it, which passed through the wall of the coil to a terminal. This branch wire was repeated after each further winding of one mile in the case of the small coil, and of ten miles in the case of the large one. By means of a plug the terminals can be readily put in connection with one wire of the circuit, the other end of which is permanently connected to the beginning of the insulated wire. By this means any resistance from 1 to 99 miles could be put in circuit.

TABLE I.

1 Number of Daniell Elements. n.	2 Reading of Sine Galvanometer.	3 Deflection of Needle. a.	4 Sin a.	5 $\frac{\sin a}{n} \cdot 100.$	6 Differences.
0	277.4				
8	282.5	5.1	0.0089	29	+0.7
4	284.1	6.7	0.1166	29	+0.7
5	285.7	8.3	0.1443	29	+0.7
6	287.3	9.9	0.1719	28	-0.3
0	277.4				
7	288.9	11.5	1.1994	28	-0.3
8	290.6	13.2	0.2283	28	-0.3
9	292.4	15.0	0.2588	28	-0.3
7	289.0	11.6	0.2010	29	-0.7
3	282.6	5.2	0.0906	30	+1.7
0	277.4				
18	307.2	29.8	0.4970	27	-1.3
12	296.7	19.3	0.3004	27	-1.3
12	296.9	19.5	0.3338	28	-0.3
21	311.7	34.3	0.5635	27	-1.3
3	282.6	5.2	0.0906	30	+1.7

In carrying out each experiment of this table, after the needle had been again brought back to zero by turning the galvanometer,

the whole resistance of 99 miles was inserted, and the position of the needle again observed; the conductor was then connected up to the gas-piping, and the wire touching the outer coating then shifted from the middle to the outermost edge of the coating; lastly, the circuit was so connected up that instead of the discharge the charging current was sent through the galvanometer; with all these alterations the deflection of the needle was not altered in the least. From column 4 it follows: that the deflection of the needle is proportional to the number of cells, and consequently to the electromotive force of the battery. The slight reduction of the calculated values for the deflection with one element with increase of the battery, continues in the later series of experiments when the mean of two readings was taken with the direction of the current reversed, and is consequently to be looked upon as an error in the instrument.

The following conclusions may hence be drawn:—

1. The charge of a condenser, or the quantity of electricity collected on its surfaces, is proportional to the electromotive force of the battery.

2. It is independent of the resistance of the connecting wires, and independent of the place where the connecting wire touches the coating of the condenser.

3. It is not altered by connecting one battery pole or one of the two coatings to earth.

The first of these conclusions requires no further comment. That the charge of a condenser is proportional to the electric force of the battery, or the density of the electricity of the inexhaustible source from which it is charged, was to be expected, and was analogous to the behaviour of frictional electricity. The second shows that the duration of the separate charges or discharges was in this case less than about $\frac{1}{150}$ of a second, i.e., the duration of a half oscillation of the commutator, even when the time of charge was made considerably longer by the insertion of a resistance of 99 miles. There is nothing unexpected either in the third conclusion; still it was very surprising to me that the position of the point of contact of the conducting wire with the insulated condenser-plate was without any influence on the amount of the deflection of the needle. On the contrary, it appeared probable to me that the charge would be at its greatest

when the middle of the coating touched the conducting wire, and that it would be all the smaller the more the point of contact was shifted to the edge. This was, however, by no means the case. The position of the needle remained quite unaffected so long as the leading wire was only in contact with the coating, even when only the outermost point of the rectangular tinfoil coating was in contact with it. I have varied the experiment in many ways with condensers and Leyden jars of the most various forms and sizes, but always with exactly the same result. For the success of my experiment, this result—that is, the independence of the charge of a condenser on the position of the conducting wire—was very important, since the experiments divested of this consideration were much more simple and reliable.

An examination of the above series of experiments removes at the same time many doubts which might be raised against the certainty of my experimental method. One of the most important would certainly be whether or not the ratio of magnetism of the astatic needles employed was permanently or only temporarily altered during the discharges. The greatest care has, indeed, always been taken to make sure against any errors arising from this cause. Only thoroughly glass-hard magnet needles of crucible steel (which is specially suitable for the production of steel magnets) were found sufficiently constant with strong discharge currents. This was quite evident, since the time of oscillation of the needles, and also their zero position remained unaltered. If a temporary change had occurred in the magnetism of the needles, the deflection would have been changed by the insertion of a considerable resistance in the circuit of the galvanometer. With very powerful batteries and very weak condensers, I have indeed observed this phenomenon, when the discharge current had only to overcome the resistance of the galvanometer wire. As the time of oscillation and the position of the needles were not thus altered, it must be assumed that the strength of the current was quite sufficient to change the magnetism of the needle, especially that of the inner one, but that the duration of the current was not, however, sufficient to render this altered magnetism permanent. That the duration of the magnetizing current is of considerable importance as regards the amount of the permanent magnetism is a well-known fact. It is hence quite conceivable that a powerful current

of very short duration, like the discharge current of a Leyden jar, may momentarily completely destroy or reverse the magnetism of a needle ; that, therefore, the electromagnetic action of this current on the needle does not occur to the anticipated degree, or hardly at all ; but that nevertheless the permanent magnetism of the needles is hardly at all or only slightly altered when the discharge current has ceased. This, then, points to the conclusion that as regards its magnetic coercive force hard steel with currents of very slight duration is similar to imperfectly elastic bodies. In order to be quite assured against such disturbances of the measurements, I have lately always inserted the resistance of 99 miles in the circuit of the galvanometer, and connected besides with the galvanometer wires, the coatings of a battery of nine Leyden jars or of another condenser of considerably greater capacity, so that the charge to be measured had to spread itself over the coatings of this condenser before it passed through the resistance coils and galvanometer wire. The discharge current was consequently of longer duration and proportionately smaller intensity. Neither the use of such a reservoir in connection with the wire of the galvanometer, nor yet the insertion of a resistance, is of any influence on the amount of the deflection of the needle, since the time of discharge is always considerably less than the time of oscillation of the commutator.

From the independence of the deflection of the needle on the amount of the resistance inserted, it could easily be concluded that the nature and position of the leading wires had no effect on it. This is, however, only the case as regards the wires leading from the condenser to the galvanometer, and not as regards those from the battery to the commutator and condenser. These last form a condenser of themselves, as will be further discussed later on, the current charging which also passes through the galvanometer and deflects the needles. I therefore altered my first arrangement and placed the commutator and condenser quite close to the battery, and always satisfied myself beforehand by means of experiments with very sensitive needles, made with the condenser excluded, of the action of the leading wires alone, and afterwards brought this into the calculation. The length and form of the conducting wires have, on the other hand, no influence on the amount of the charge of the condenser itself. If I measured the charge of a certain

condenser with short leading wires, and afterwards employed as battery wire a copper wire 1^{mm} thick and 50 metres long, freely suspended between the buildings of the station, the charge increased with an unchanged battery, quite independently of the capacity of the condenser by a constant amount, which exactly agreed with the charge of the leading wire alone.

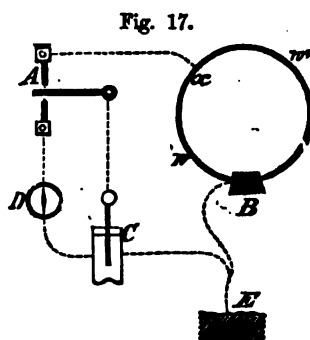
To make more certain of my method of research, and at the same time also to show experimentally that my assumption that the charge at each point of the surface of a core connected to earth, was proportional to the electric force or density at this point, according to Ohm's electroscopic law, I made the following series of experiments :—

TABLE II.

1 Number of Elements. <i>n</i> .	2 <i>w</i> .	3 <i>w'</i> .	4 Reading.	5 <i>a</i> .	6 Sin $\alpha = Q$. Observed.	7 Calculated.	8 Differences.
8	200	800	305·6 256·0	24·8	0·4193	0·424	+0·006
8	300	700	258·8 305·5	22·8	0·3794	0·371	-0·008
8	400	600	299·6 262·4	18·6	0·3189	0·318	0
8	500	500	265·4 296·2	15·4	0·2655	0·265	0
8	600	400	268·8 294·0	12·6	0·2180	0·212	-0·006
8	800	200	275·6 287·6	6·0	0·1045	0·106	+0·002

The arrangement of circuit used for this purpose is shown in Fig. 17. The circle *ww'* is the conducting wire; the battery B is permanently connected up to it; the tongue of the commutator A is in connection with the knob of a Leyden jar C, and one contact of the commutator with the galvanometer D. The other galvanometer wire, the outer coating of the jar, and one pole of the battery are connected together and with the earth. The wire connected with the other contact of the commutator is led to the point of the circuit, of which the electroscopic force is to be measured. If E is the electromotive force of the battery, then,

according to Ohm's law, the electric force $x = C \frac{w'}{w + w'}$, when w and w' represent the resistances from the point of the wire where x is to be measured up to the battery. As a test wire I made use of a resistance coil of covered German-silver wire, the resistance of which was almost exactly divided into ten parts, the resistance of each of which was equal to that of an iron telegraph wire 2^{mm} thick and 100 Russian versts long. Column 1 gives the number



of cells ; 2 and 3, the resistances w and w' ; 4, the readings of the divided circle of the sine galvanometer ; 5, the half-difference of the same, consequently the measured angle of deflection α ; 6, the sine of this angle, which corresponds with the amount of the charge of the jar, and consequently with the density x ; 7, the values calculated according to the formula. The constant is calculated for each measurement and the mean of all taken ; it is equal to 0.53. The close agreement of the observed values proves the correctness of my assumption, and increases one's confidence in the method of measurement employed.

I now passed to the determination of the dependence of the charge on the form and size of the condenser.

The following series of experiments is made with a battery of nine jars, each of which had an internal coating of 13 square decimetres and 4^{mm} thickness of glass ; n represents the number of Daniell cells employed ; s the number of jars ; α the measured angle of deflection.

TABLE III.

Number of Elements used. n.	Number of Jars. s.	Deflection of Needle. a.	Sin a.	Sin $\frac{a}{n \cdot s}$.
8	9	32.2	0.5328	0.074
8	8	27.5	0.4630	0.072
8	7	24.4	0.4130	0.073
8	6	20.8	0.3550	0.073
8	5	16.4	0.2823	0.070
8	4	13.2	0.2283	0.071
8	3	10.0	0.1736	0.072
16	3	20.7	0.3534	0.073
18	5	42.3	0.6729	0.074

The sufficiently constant values of the last column prove that the charge of a battery of many jars is in proportion to the product of the number of jars into the electromotive force of the battery, as was to be expected. When I placed the jars in series, instead of in three series close to one another as usual, the amount of charge was not altered in the least. This proved to be a confirmation of the independence of the charge on the disposition and form of the leading wires already observed, and made it appear more likely to me that with electricity of low tension the capacity of the condenser was dependent only on the amount of the surface, provided the thickness, nature, and form of the insulating material remained unaltered. I have found the absolute correctness of this assumption fully proved and everywhere confirmed. To avoid repetitions, I defer nevertheless the experimental proof of this point, since the experiments made thereon will serve at the same time to answer some questions to be discussed later on.

In order to discover the influence of thickness of the insulating layers separating the two parallel coatings of a condenser, I had several plates of glass 1^{mm} thick prepared, ground almost parallel, which were all 0.26^m long and 0.21^m broad. I satisfied myself that the thickness of these plates was sufficiently exact by a gauge which measured with accuracy to $\frac{1}{100}$ ^{mm}. Two such plates were provided with two tinfoil coatings, exactly opposite to one another, 0.34^m long and 0.18^m wide. The measured charge gave for both almost exactly the same capacity. Two other plates were now provided with a tinfoil coating of the

above dimensions only on one side. One such plate covered on one side was placed upon insulated supports 6" high, and its coating connected with the tongue of the commutator. If now a battery pole was placed in connection with the one, a galvanometer wire with the other contact screw of the commutator, the needle was deflected, and the amount of the deflection was proportional to the number of cells of the battery employed. The deflection diminished when the glass plate was kept as free as possible in the middle of the room, and was greater the nearer it was brought to its walls. With a very sensitive pair of needles and a battery of 54 Daniell cells, I could show the same phenomenon with almost any insulated conductor, which was put into connection with the oscillating tongue of the commutator. I was therefore in a position to measure by means of the galvanometer, the so-called free electricity which was accumulated by the electric potential of the insulated pole of the battery on the upper surface of a chosen conductor, and to compare it quantitatively with a static charge of electricity. The important influence of the greater or less proximity of the walls on the quantity of this free electricity made it appear very probable to me that it was entirely a charge between the conductor and the conductive walls of the room in which it was placed, just as Faraday is known to have assumed it to be.

The charge of a conductor evidently consists then of two parts, the charge between the insulated coating and the walls of the room, and that between the insulated and non-insulated coating. The galvanometer measures the sum of both these. To find the latter, I managed so as first to ascertain the free electricity of the insulated coating, and then the total charge, whilst the second coating which was previously insulated was connected to earth. From this total charge, half the charge of the insulated coating was deducted. That only half is to be deducted follows from the consideration that the coating connected to earth may be considered thick enough to reach to the wall of the room, without the charge being thereby increased; so that there only remains the amount of the charge of the side of the insulated coating turned away from the walls of the room to be taken into account.

To explain the following series of experiments with seven glass plates 1^{mm} thick, I have to remark that the plates, thoroughly

cleaned, were moistened with rectified oil of turpentine, and then well rubbed together to remove any adhering air. The plates so prepared were placed between two plates of vulcanized india-rubber, and loaded with a 10-pound metal plate. To determine the charge between the coatings of a single glass plate, it was provided with a second coating fixed to the glass in the usual way.

TABLE IV.

1	2	3	4	5	6
Number of Plates. <i>m</i> .	α .	$\sin \alpha$.	$\sin \alpha - \frac{\sin 0.5^\circ}{2}$.	$\left[\sin \alpha - \frac{\sin 0.5^\circ}{2} \right] m$.	Differ- ences.
I.	35.8	0.5850	0.5807	0.58	-0.02
II.	17.3	0.2974	0.2931	0.58	-0.02
III.	11.8	0.2044	0.2001	0.60	0.00
IV.	8.9	0.1547	0.1504	0.60	0.00
V.	7.1	0.1221	0.1178	0.59	-0.01
VI.	6.2	0.1079	0.1036	0.62	+0.02
VII.	5.5	0.0958	0.0915	0.64	+0.03

The first column of the above table gives the number m of the glass plates between the coatings acting inductively on each other, α is the measured angle of deflection of the sine galvanometer. A single, insulated coating gave the deflection 0.5° . As already explained, half the sine of this angle must be subtracted from $\sin \alpha$. Column 5 shows that the charge is inversely proportional to the number of glass plates, and consequently to the thickness of the insulating layer. The differences do not exceed the limit of accuracy to be attained by the means employed. Their slight increase with thicker glass indicates, however, an increase of charge at the boundaries of the coatings, such as must occur if molecular induction exists, according to Faraday's supposition.

The following series of experiments was made with six plates of gutta-percha rolled as uniformly as possible, provided with tin-foil coatings on both sides. These plates were so placed in layers over one another, that all the tin-foil coatings were exactly over one another. Between each two plates a projecting strip of tin-foil was laid, which served to make connection with the respective coatings. The plates were squeezed by means of a hand press

between two smooth boards and elastic india-rubber plates, and in this condition the measurements were made. The charge between each two neighbouring coatings was first measured, and then the charge between the first and all the rest of the series.

TABLE V.

Number of Gutta-percha Plates used.	1	2	3	4	5	6
Deflection of Needle . .	19.5	19.5	19.3	19.9	19.1	18.3

TABLE VI.

Charge between the Coatings. m.	α .	$\sin \alpha$.	$\sin \alpha - \frac{\sin 0.9}{2}$.	$\left[\sin \alpha - \frac{\sin 0.9}{2} \right] m$.	Differences.
1 and 2	19.8	0.3386	0.3308	0.33	-0.05
1 and 3	10.9	0.1891	0.1813	0.36	-0.02
1 and 4	7.5	0.1305	0.1227	0.37	-0.01
1 and 5	6.5	0.1045	0.0976	0.38	+0.01
1 and 6	5.0	0.0872	0.0794	0.40	+0.02
1 and 7	4.3	0.0750	0.0672	0.40	+0.02

From the first table (V.) it follows that the capacity of the condenser formed of six gutta-percha plates was very uniform. The sixth plate only gave a noticeably smaller charge. The measured charge of a single coating gave 0.9° ; for that reason in Table VI. $\frac{\sin 0.9}{2}$ was deducted from the sine of the measured angle α . In

the last column but one is given the products of this corrected measure of the charge into the number of gutta-percha plates between the active coatings, and in the last column their differences. A small increase of the differences with the distance of the condenser plates is still more apparent in this case than in the experiments with glass plates, which is fully explained by molecular action of the electrostatic induction in curved lines between the edges of the coatings.

A most important question, the solution of which also determines that of the existence of induction in curved lines, is that of the influence on the amount of electrostatic induction of the

insulating material which fills the space separating the two condenser plates. That the capacity of a condenser is evidently dependent on the material of the separating insulator is settled beyond doubt by Faraday's experiments, confirmed elsewhere. On the other hand, former experiments did not decide whether Faraday's view is correct, that electrostatic induction is exclusively an action propagated from molecule to molecule of the separating insulator, or whether rather the influence of the insulating material is a secondary one, and that, perhaps, direct induction and molecular induction act simultaneously. That a penetration of the electricity of the coating of a Leyden jar actually takes place into the substance of the glass, and hence the distance from one another of the mutually attracting electricities is diminished, is abundantly proved, and follows also from the fact that a fully discharged jar, which was charged a long time previously, after a short time again becomes charged. The question then is whether :

1. The influence of the insulating material filling the separating space still shows itself, when the penetration of electricity into its mass is impeded, or the experiment is so arranged that it can exert no influence upon the result of the measurement. Further, if this is the case, whether :

2. Electrostatic induction entirely obeys the law of molecular attraction, or entirely or partly that of attraction at a distance.

Experiments with voltaic electricity appear to me specially suitable for answering this question, for it presents a source of electricity always constant and inexhaustible, by which all measurements would be much simplified. The above-mentioned experiments will have proved this very convincingly, as well as the accuracy of the galvanometer results.

That with voltaic electricity also, the capacity of a condenser depends essentially on the material of the insulator, which fills the separating space of the collector plates, may be easily admitted.

When two round flat discs, 15^{cm} in diameter, were kept separate by a plate of glass 1^{mm} thick, the galvanometer showed nearly double as great a charge as when, instead of the glass plates, small pieces of glass of the same thickness were placed between the discs.

This increase of charge took place to the same extent whether

strong or weak batteries were used, and was therefore independent of the effective strength of the battery.*

As the extent of the eventual penetration of the electricity in the time given by the commutator must in any case be dependent on the magnitude of the effective force, the conclusion may be drawn that the cause of the observed increase of the charge is not to be found in this. This is shown still more certainly by the following experiment :

I had a Leyden jar made of two glass cylinders, placed within one another. The inner one was 0·57^m high, and had an internal diameter of 0·18^{cm}, and the outer one was the same height, with an external diameter of 0·20^{cm}. The concentric space between the two cylinders was about 15^{mm}. The thickness of the glass of each cylinder was on an average 2·45^{mm}. The cylinders were fixed with colophonium cement to a base plate, and the interior filled in to a height of 1 inch with melted cement. The inner and outer surfaces of the double cylinder were covered with tin-foil to within $\frac{1}{2}$ ^{dm} of the upper and lower edge, and the rest was covered over with insulating lac in the usual manner. The charge of the jar was now measured under otherwise similar conditions when the space between the cylinders was filled with air, and when it was partly or altogether filled with different insulating substances. If a penetration of the electricity into the glass now took place, owing to which the charge would be sensibly increased, this penetration could not possibly reach the insulator in the middle of the thick glass already insulating by its simple thickness. Nevertheless a considerable increase in the charge took place when a solid insulator, for instance a cylinder of india-rubber, or well insulating gutta-percha, was placed between the glass walls. This very decided experiment can also be more simply arranged, and with the same result, with two glass plates, which are coated on one side, and placed at such a distance from one another that a third glass plate can be passed between them, without altering the distances of the coatings from one another.

* I have preferred to use the expression "electric force" instead of "electromotive force," because the experiments in question only depend upon the electroscopic strength or potential of the electricity of the pole of the battery, not, as in true galvanic phenomena, on the result of this force, viz., the electric current. The expression "density of the electricity" has quite a different meaning, and cannot be employed here.

The mode of proceeding described was not suitable for determining the dielectric constant of different insulators. I obtained, however, tolerably constant measurements in the following manner: Two round flat plates of brass of 15^{cm} diameter were ground exactly to one another. By means of three screws with fine threads which passed through the upper of the two discs, they could be separated from one another to any desired degree. The ends of the screws had stones set in them, and the discs were thus insulated from one another. These discs were then placed under the receiver of an air pump. The lower one was connected with the metal plate of the air pump, the upper with an insulated wire passing through the plate. After having made connection with the commutator and having observed the deflection of the needle, I pumped out the air to within two lines of the mercury manometer. The position of the needle was not altered in the least, nor was an alteration observable when the receiver of the air pump was filled with carbonic acid or hydrogen. It was thus clearly proved "that gases of every kind and density have the same dielectric constant."

As was to be expected, the position of the needle was not altered by warming the plates and the air separating them, when the heating was not carried so far as to distort the plates. By turning the screws equally, the plates of the condenser were placed at a parallel distance apart of about 1^{mm}. A flat-bottomed vessel was then filled to a height of from 1 to 1½ inches with the fusible insulator which was to be examined, and the mass slowly melted. After the upper surface had been well cleaned, the amount of the charge between the two discs in air was first measured, and then one after the other was plunged into the melted mass, so that no air bubbles remained between the plates. The ratio of the charges measured gave the dielectric constant of the insulator that was tested. There was thus obtained for stearine the number 0·78, and for sulphur 2·9; after the mass had cooled the numbers were smaller, which might have been caused by the upper plate becoming slightly raised by the crystallisation. The numbers found for the dielectric constant of sulphur may moreover have been somewhat increased by the brass plate becoming covered with a thin layer of conducting sulphide of copper.

I examined with greater exactness the dielectric constant of gutta-

percha and glass. This I did in the following manner : a circular flat plate of gutta-percha was laid on the lower condenser plate, having been provided with three holes, through which the screws of the upper plate passed. After the latter was pressed down hard on the gutta-percha plate, and loaded with a weight of 10 pounds, the screws were turned round until their stone points rested on the lower discs. After the deflection of the needle was observed, the plate was raised, the gutta-percha removed, and the measurement repeated. The same thing was done with flat ground-glass plates.

When I warmed a glass plate, which gave an unexpected deflection, so as to remove the damp supposed to be on its upper surface, I was surprised to find a decided increase in the deflection. The amount was increased ten-fold by heating up to the melting point of tin, and rose to 30 times the original amount by further heating up to the melting point of lead. If the upper plate was moved on one side over the glass, the needle which previously had a deflection of 3° struck the stop, and then went back again to 30° or 40°. I was at first inclined to assume an increase in the dielectric constant of glass due to heating, but I afterwards felt satisfied that this phenomenon had been brought about by electrolysis of the substance of the glass.

It has already been settled by the experiments of Buff and Beetz that glass becomes conductive with slight heating. The charging currents must therefore have evidently been greater, as the galvanometer showed simultaneously the strength of the current passing through the glass. The discharge currents must on the other hand have been weakened by conduction of the substance of the glass, as the charge could equalize itself not only through the galvanometer but also through the substance of the glass. The great increase in the discharge current which was observed can hence apparently only be sought for in the electrolytic separation of metallic potassium or sodium at the coating acting as the negative anode. Fused common salt and other electrolytic salts gave very similar results. Very strong polarization phenomena also occurred with these, and even continued, after the salt had crystallized again, only ceasing when it had become quite cold. Hard potash glass began to be conductive at about 40° C., soft white-soda glass still earlier. With a plate of such glass I found a diminished decrease in the discharging deflection of the needle,

even as low as 5° . Heating had no influence on the charge of mica plates, and the insulation remained perfect, even with the greatest possible practicable heating. Gutta-percha on the other hand, even with slight heating, showed considerable conductivity. When I dipped a copper wire 5^{dm} long covered with gutta-percha into a vessel of cold water right up to its free end, it appeared perfectly insulated. When on the other hand I dipped it into water, warmed to about 40° C., the galvanometer showed a deflection of about 6° , which disappeared when the warm water was changed for cold. The increase of the charging current was on the other hand very much less in this case than in that of heated sheets of glass, which may be explained by the fact that potassium and sodium are much higher in the electric potential series than hydrogen.

From these experiments it appears probable to me that all those solid electrolytic bodies which conduct electricity in the melted state lose their insulating power already in the solid condition when they approach their melting point, and that they are better insulators the lower their temperature is below their fusing point.

The previous experiments admit of no more doubt on the point that the influence of the insulating material depends on the amount of electrostatic induction, even with electricity of very low tension, and that it is not to be explained by the penetration of electricity into the substance of the insulator. If this is granted it is hardly possible to explain the considerable increase of induced electricity with the use of solid insulators, otherwise than by the acceptance of Faraday's hypothesis of molecular induction. It may be quite well imagined that direct induction takes place as well by direct action at a distance as by molecular action. In order to make certain of this, I took several coated glass plates 1^{mm} thick and connected the lowest with earth. If they were connected up according to the arrangement given in Fig. 17 the constant deflection of the needle was a measure of the charge of the condenser. If instead of the second the third coating was connected with the commutator, the charge was only about half as great as resulted from the previous experiments. I now joined both the second and third coating with the tongue of the commutator. As in this case both coatings were electrified the deflection must be greater when the third coating exercised through the

second an inductive influence on the natural electricity of the coating put to earth. But this did not entirely take place. Even when five coatings were connected with the tongue of the commutator the deflection remained exactly as great as with one coating.

I have yet to remark that I had made the second coating somewhat greater than the rest. When this was not the case, I obtained a less increase of the deflection, which is easily explained by induction in curved lines.

I obtained the same result when I placed three jars provided inside and outside with tinfoil within one another. When the outer coating was put to earth, I obtained exactly the same charge, when the next coating alone, or when the other two were simultaneously connected with the tongue of the commutator. It demonstrates this any way—that the inductive force does not act through an equally strongly electrified conductor, and makes it very probable that direct induction, even if it exist, is exceedingly small compared with molecular.

The results hitherto obtained show that the quantity Q of electricity taken by a condenser, formed of two parallel opposite flat plates of equal size, is directly proportional to the electric force E of the battery, directly proportional to the areas F of the opposing surfaces, inversely proportional to the thickness d of the insulating layer, and directly proportional to a constant k , which depends upon the material of the insulator employed, consequently the charge is—

$$Q = E \frac{F k}{d} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

with the limitation, that d is very small as compared to F , or a correction must be applied which compensates the effect of the probable induction in curved lines between the edges of the coatings and around them. The form of this equation agrees perfectly with the law of motion of heat and electricity in conductors. If the insulator is considered as a conductor, the areas F , the leading wires, and the battery itself as without resistance, then the current would be—

$$J = E \frac{F \lambda}{d}$$

if λ represents the coefficient of conductivity of the material of the

plates separating the surfaces. According to this, one may represent the charge as arising from a current of very short duration through the mass of the insulator, and may give to the above equation the form—

$$Q = \frac{E}{V}$$

where V represents the expression $\frac{d}{F K}$, for which I propose the expression “inductive resistance,” for it is quite analogous to the conductive resistance of the space across which the induction acts.

If electrostatic induction is exclusively a molecular action, as it must at least very probably appear to be according to the previous results, then the equation $Q = \frac{E}{V}$, the correctness of which has hitherto only been proved experimentally for the one case in which the condenser consists of two parallel surfaces, the distance apart of which is very small compared with the dimensions of the collecting plate, must be universally true. As a preliminary test the application of the formula to the Franklin or cascade battery appeared to me specially suitable.

If the measured charges of a number of condensers of different capacities are represented by $q, q', q'',$ etc., and the “inductive resistances” by $v, v', v'',$ etc., then according to the law of induction given above—

$$q = \frac{E}{v}, q' = \frac{E}{v'}, q'' = \frac{E}{v''}$$

and hence—

$$v = \frac{E}{q}, v' = \frac{E}{q'}, v'' = \frac{E}{q''}.$$

If Q represents the charge of the collector plates connected as a cascade battery, and V the consequent inductive resistance of this battery, we have further—

$$Q = \frac{E}{V}, \text{ and } V = \frac{E}{Q}.$$

The inductive resistance of the collector plates jointly is the sum of the resistance of each. Hence—

$$V = v + v' + v'' \text{ etc.}$$

$$\frac{1}{Q} = \frac{1}{q} + \frac{1}{q'} + \frac{1}{q''} \text{ etc.} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$Q = \frac{1}{\frac{1}{q} + \frac{1}{q'} + \frac{1}{q''} \text{ etc.}}$$

To test the correctness of this formula for the charge of the cascade battery, I provided three of my glass plates of 1^{mm} thickness, with coatings of different sizes. The plate represented by I. has coatings on both sides, which form a square having sides 20^{cm} long, and are exactly opposite. The coatings of plate II. had sides 14^{cm} long, and those of plate III. sides 10^{cm} long, The charge of the separate collectors was first measured, then that of the different cascade batteries made by combining them.

Table VII. B. gives experiments with three other plates of 20 18 and 15^{cm} length of side.

TABLE VII.—A.

1 Mark on the Collector.	2 Observed Deflection. a.	3 Sin α = Q.		5 Differences.
		Observed.	Calculated.	
I.	36.8	0.599		
II.	18.1	0.309		
III.	9.7	0.168		
I. and II.	12.5	0.216	0.20	0.01
I. and III.	8.4	0.146	0.13	0.01
II. and III.	7.0	0.122	0.11	0.01
I., II. and III.	6.4	0.111	0.09	0.02

TABLE VII. — B.

1 Mark on the Collector.	2 Observed Deflection. a.	3 Sin α = Q.		5 Differences.
		Observed.	Calculated.	
I.	20	0.342		
II.	15.3	0.263		
III.	9.6	0.166		
I. and II.	8.2	0.142	0.140	0.002
I. and III.	6.5	0.113	0.112	0.001
II. and III.	5.8	0.101	0.097	0.004
I., II. and III.	4.6	0.080	0.076	0.004

Column 1 gives the arrangement of the charged condensers, whether separate or combined in cascade fashion. Column 3 gives the observed jar-charge q of the separate condensers, and the observed total charge of the combined batteries. Column 4 shows the charges of the combined batteries calculated from the formula—

$$Q = \frac{1}{\frac{1}{q} + \frac{1}{q'} + \frac{1}{q''} + \text{etc.}}$$

The differences are not to be explained by errors of observation alone. The calculated values are all somewhat smaller than the observed, and the more so, the more condensers were combined. This can be easily accounted for, since the charge between the coatings and the walls of the room could not be taken into account.

The measurement of the charge of condensers with coatings of different sizes, gives an opportunity of establishing the proof up till now left incomplete, that the charge of two condensers of different thickness of glass is proportional to the area of the surfaces opposed to each other, provided that the effect of the induction in curved lines at the edges is taken account of.

TABLE VIII.

Mark on the Collector.	Deflection. α .	Sin α .	Area in sq. c.m. F .	$\frac{\text{Sin } \alpha}{F}$.	Differences.
I.	20	0.3420	400	85	5
II.	15.3	0.2638	324	81	1
III.	9.6	0.1666	525	74	6
				Mean 80	
IV.	36.8	0.5990	400	15	1
V.	18.1	0.3099	196	16	0
VI.	9.7	0.1684	100	17	1
				Mean 16	

Column 5 of the above table gives the quotients $\frac{Q}{F}$ for six condensers of similar thickness of glass. As may be seen these values still vary somewhat widely. The differences are indeed pretty considerable, but are to be explained by the dissimilarity of the

glass plates, and especially by the induction in curved lines between the edges and between the surfaces which are not opposed to one another.

Leyden jars of different size and cores are more suited than plate condensers for confirming the formula—

$$Q = \frac{1}{\frac{1}{q} + \frac{1}{q'} + \frac{1}{q''} + \text{etc.}}$$

for charging cascade batteries. The experiments made with such batteries are collected in Table IX.

TABLE IX.

Mark on the Jar.	α .	$Q = \sin \alpha$.		Differences.
		Observed.	Calculated.	
I.	21.2	0.3616	...	
II.	11.8	0.2044	...	
I. and II.	7.6	0.132	0.130	+0.002
III.	24.4	0.4130	...	
IV.	13.6	0.2350	...	
V.	7.2	0.1253	...	
III. and IV.	8.9	0.154	0.150	+0.004
III. and V.	5.6	0.095	0.096	-0.001
III., IV. and V.	4.1	0.071	0.068	+0.003
VI.	27.3	0.4586	...	
VII.	14.9	0.2571	...	
VIII.	7.9	0.1374	...	
VI. and VII.	19.8	0.170	0.165	+0.005
VII. and VIII.	5.3	0.092	0.089	+0.003
VI. and VIII.	6.2	0.107	0.105	+0.002
VI., VII. and VIII.	4.5	0.078	0.075	+0.003
A.	14.4	0.2487	...	
B.	11.0	0.1908	...	
A. and B.	6.5	0.113	0.108	+0.005

The experiments separated by intervals were made at different times and are hence not directly comparable. All the calculated values, but one, are somewhat smaller than the observed values, as was to be expected, for the charge of the conducting wires and of the outer coatings of the jars in relation to the walls of the room is not taken into consideration. Jars I. to VIII. were most different in form and thickness of glass. The cascade batteries

were always formed by connecting the outer coating of one jar with the knob of the next. All the jars are insulated from one another by standing on a base of ebonite. The jars denoted by A and B consist of wires each 1^{mm} thick and 80^m long, which were simultaneously covered with gutta-percha. The section of the gutta-percha was approximately an ellipse. The axes of the copper wires of the core A were 2.75^{mm} distant apart, and the diameters of the gutta-percha coatings were 8^{mm} and 9^{mm}. The distance of the wires of core B was 4^{mm}, and the diameters of the gutta-percha covering 10^{mm} and 18^{mm}.

I have proposed double wires of this kind for long submarine lines, and shall return to them repeatedly later on.

The proportion of the charge of these cores will serve as a further proof of the correctness of the law of induction. According to Kirchhoff,* the resistance between two circles in an unlimited plane is—

$$W = C \log \frac{a}{r},$$

where a is the distance between the centres of the circles, r their radius and C a constant. The same formula should also be applicable for the inductive resistance; and consequently—

$$\frac{\log \frac{a}{r}}{\log \frac{b}{r}} = \frac{\frac{1}{0.248}}{\frac{1}{0.191}} = \frac{0.191}{0.248} = 0.77$$

where a and b are the distances, and r the radius of the wires. By substituting the numerical values we obtain—

$$\frac{\log \frac{a}{r}}{\log \frac{b}{r}} = \frac{\log \frac{2.75}{0.5}}{\log \frac{4}{0.5}} = 0.81.$$

The equation is consequently pretty well satisfied when it is taken into consideration that the wires are not exactly parallel, that the formula for the resistance is only an approximate one, and that it is only correct for the case of an unlimited plane of equal conductivity throughout, which is not here the case; as the inductive

* Pogg. Ann., Vol. LXIV., p. 497.

resistance of gutta-percha is only half as great as that of air, the differences between the observed and calculated values may even be considered remarkably small. In Table X., below, the experiments are collected which I have made on the charge of cores of different dimensions. They were pieces of wire made at different times, and the gutta-percha was in places somewhat changed by contact with the air. Although the most serviceable wires were used, the centring of the wire in the core is more or less imperfect. Hence great exactness could not be expected from these experiments. The experiments were so arranged, that out of the wires to be tested, coils of about one foot internal diameter were made which were placed in a metal vessel filled with water. One end of the wire projected out of the water, and the other placed in the water, was previously insulated by surrounding it with heated gutta-percha. The wire was connected with the tongue of the commutator; the connection with the water was made through the metal vessel.

TABLE X.

1 No.	2 l .	3 r .	4 R .	5 a .	6 $\sin \alpha = Q$.	7 $\frac{Q}{l} \cdot \log \left(\frac{R}{r} \right)$.
1	7.5	0.8	3.4	16.6	0.2856	0.024
2	5.5	0.8	2.1	16.3	0.2806	0.021
3	6	0.6	2.2	11.8	0.2044	0.019
4	6	0.8	2.2	15.3	0.2638	0.019
5	3.3	1.0	3.7	7.6	0.1322	0.022
6	7.6	0.8	2.1	25.2	0.4258	0.024
7	7.6	0.8	3.4	18.6	0.3189	0.025
8	2.9	1.0	2.5	5.5	0.0958	0.019
9	34.4	0.8	2.5	39.2	1.8960	0.027
10	7.5	0.8	3.4	19.1	0.3272	0.025
11	8.3	0.8	2.6	28.7	0.4802	0.027
12	2.9	1.0	3.8	5.6	0.0975	0.019
13	3.2	0.9	3.2	6.6	0.1149	0.018

Column 2 gives the length l , column 3 the radius of the metal wire, column 4 the radius of the covered wire, column 6 the measured charge, and column 7 the constant calculated from it for the formula for the charge explained later on. The experiments separated by a space have been made at different times and are not comparable. Experiment 9 was made with a wire of con-

siderable length covered with lead. In this case the wire formed the inner, the lead the outer coating of the condenser. As the charge of this wire could no longer be measured with the Daniell battery of 54 cells, which was used for the other measurements, a battery of 18 cells was used, and the sine of the angle measured multiplied by three. The differences are certainly very considerable, but may be explained by the nature of the wires examined.

The calculation of the charge of the cores is made according to the formula—

$$Q = \frac{E C}{\log \frac{R}{r}}.$$

This follows from equation, $Q = \frac{E}{V}$, if the cylindrical gutta-percha coverings are supposed to be divided into a large number of concentric layers and their resistances totalled.

If dx is the thickness of such a hollow cylinder of radius x , its resistance is—

$$dv = \frac{dx}{2 \pi x k l}$$

where k represents the dielectric constant of the gutta-percha, hence—

$$v = \frac{1}{2 \pi l k} \int_r^R \frac{dx}{x} = \frac{1}{2 \pi l k} \log \frac{R}{r}$$

and

$$Q = \frac{E}{V} = \frac{E 2 \pi l k}{\log \frac{R}{r}} \quad . \quad . \quad . \quad (4)$$

or, if E remains unchanged—

$$Q = C \cdot \frac{l}{\log \frac{R}{r}} \text{ and } C = \frac{Q}{l} \cdot \log \frac{R}{r}.$$

* Wm. Thomson has obtained in another way the value $\frac{k}{2 \log n} \cdot \frac{R}{r}$ for the capacity of unit length of a core. As up till now I have only seen an abstract of his work, I cannot say why the constant ($\frac{1}{2}$ in place of 2π) of Thomson's formula differs from mine.

Although the agreement of the constants thus calculated is not satisfactory, at least it certainly follows from them, that the induction does not follow the law of direct attraction. The attraction of parallel lines, the length of which is infinite or at least very great in comparison with their distance apart, is in the inverse ratio of that distance. The cylindrical covering which concentrically surrounds a thin wire may be assumed to be divided into a great number of smaller layers. The sum of the attractions between the wire and all the layers forms the sum of the effective attracting forces between the cylindrical covering and the wire, and must be the measure of the amount of the induction, if this were the effect of attraction at a distance. As now a cylindrical covering of double diameter may be divided into double as many strips of similar thickness, each of which is attracted with half as great a force by the axis, the total attraction between the axis and the covering would consequently be independent of the diameter of the cylinder, and this would also be the case with the charge, which it manifestly is not.

The incomplete realization of the equation—

$$Q = E \frac{2 l \pi k}{\log n \frac{R}{r}}$$

is partly to be sought in the eccentric position of the wire in the gutta-percha, but mostly, however, in the heterogeneity of the latter, and its being much dried and hardened in the case of many of the wires. In consequence an air-filled space was formed between the wire and the gutta-percha. Some of the wires were covered with vulcanized gutta-percha. The layers of this gutta-percha lying nearest to the copper became conductive by taking up sulphide of copper, by which the active diameter of the copper was somewhat increased. Exact figures are hence not to be expected in these cases.

In Table XI., p. 122, I have collected some experiments, the unexpected results of which first induced me to carry out the investigations now presented. I hoped to remove to a great extent the troublesome charges in long submarine lines, and the retardation of the current caused by them, by employing twin wires placed in a common gutta-percha core, and thus forming a complete metallic

circuit instead of using single wires, and the earth as a return, or reservoir, if the expression is preferred. As in this case both wires, at equal distance from the battery, were equally and oppositely electrified, I thought that the induced electricity which

TABLE XI.

1	2	3	4	5	6
No.	Number of Elements. n.	Description and Connection of the Wires.	a.	Sin a.	C. $\frac{\sin a}{n}$.
I.					
1	54	1 ÷ T	38.7	0.6252	115
2	18	do.	11.9	0.2062	
3	54	(1 + 2) ÷ T	70.5	0.9367	172
4	18	do.	18.2	0.3123	
5	54	1 ÷ 2	28.5	0.4772	86
6	18	do.	8.8	0.1529	
7	54	1 ÷ (2 + T)	43.0	0.6820	125
8	18	do.	13.0	0.2250	
II.					
1	54	1 ÷ T	33.7	0.5548	101
2	18	do.	10.3	0.1787	
3	54	(1 + 2) ÷ T	59.0	0.8572	155
4	18	do.	16.0	0.2756	
5	54	1 ÷ 2	23.7	0.4019	72
6	18	do.	7.2	0.1253	
7	54	1 ÷ (2 + T)	36.3	0.5919	106
8	18	do.	10.7	0.1856	

arises on the outer surface of the common gutta-percha covering must be zero at all those points which were equally distant from the equally and oppositely electrified wires. It must then be proportional to the difference of the inductive action of both wires at the other points, and the charge of the whole twin wire must consequently be very much less than that of a single wire. But experiment shows that this is not at all the case. There is not only no reduction, but on the contrary a slight increase of the charge in the above direction.

The measurements of the previous table are made with the

twin wires described, immersed in a vessel containing water which was connected to earth. One end of all the wires was insulated by covering it with heated gutta-percha; the other projected from the water.

Column 2 gives the number of Daniell cells n ; Column 3, the description of the wires and their connections. Heading I. in this column distinguishes the twin wires 2.75^{mm} apart; II. those at 4^{mm} apart. The Arabic numbers 1 and 2 represent the single wires of a twin wire; the sign \div between two wires that the charge is measured between the wires so represented; T signifies the connection to earth.

By $1 \div T$ is hence expressed that the charge is measured between 1 and the outer conducting covering of the gutta-percha, which is to earth; $1 \div 2$ expresses, on the other hand, that the charge is produced by joining up the battery between wire 1 and 2, without any connection to earth; finally $(1 + 2) \div T$ that the charge is measured between the two connected wires 1 and 2 and the earth. Column 4 gives the charge; Column 6, the mean of both measurements reduced to the charge by one cell (see Table XI.).

As with charge $1 \div 2$, the battery is directly joined between the two wires, and no earth connection exists; the electric force of both battery poles is equally great, and half as strong as the electric force of the insulated pole of the same battery, when the second pole is to earth.

In these experiments, therefore, the charge of each of those wires is only produced by half the number of specified cells. This becomes clearer, if the middle of the battery is imagined to be earthed. If the two wires were then simultaneously connected to the two free opposite poles of the electric battery, the charge must take place just as in the previous case. In order to be able to compare the charges of the different combinations, the charges $1 \div 2$ must be doubled. As this number is greater than the charge $1 \div T$ of the same twin wire, it follows that no reduction, but rather an increase, is brought about by the combination $1 \div 2$. This is also quite correct, according to the molecular theory of induction. Each point of the small axis of the ellipse which a cross-section of the gutta-percha forms is equally removed from the two equally and oppositely electrified wires. For the electric

current between these wires, the plane passed through all small axes is to be considered as completely earthed, for, according to Ohm's law of potential, the electric force in the whole plane is equal to 0. It hence directly follows that the current between the two wires must be stronger than between one wire and the periphery of the gutta-percha, when there are equal electrical forces in both cases in the wires. And the same must be the case as regards the charge according to the law of electrostatic inductive resistance put forward.

I have no doubt that a more expert mathematician will yet succeed in proving the correctness of the law of induction which has been put forward for all the measurements given in the table.

With this object I have communicated them fully here, and because, in the second part of this investigation treating of the retardation of the current due to the charge, I shall return to these measurements.

Some years ago I had already found that long well-insulated overhead telegraph wires were also charged by the galvanic current. I have often even succeeded in discovering the place where the conductor was broken by the amount of the discharge current. For the more exact determination of the capacity of the condenser formed by an overhead telegraph wire and the earth, I suspended at my works an iron wire two English lines in thickness and 120·85 metres in length. The wire was stretched in a great curve, and at an average height of 8 metres above the ground; the points of attachment were carefully insulated, and one end joined directly to my instrument. I now compared the charge of this wire with that of a plate condenser of 1^{mm} thickness of glass and 2·25 square centimetres of coated surface. I obtained the following result:—

TABLE XII.

Number of Elements. n.	Description of Collector.	α .	$\sin \alpha$.	$\frac{\sin \alpha}{n} \cdot c$.	Mean.
18	Iron wire	2·2	0·0383	2127	2138
36	"	4·4	0·0767	2130	
54	"	6·7	0·1166	2159	
18	Condenser	3	0·0523	2905	2948
36	"	6·1	0·1062	2950	
54	"	9·3	0·1615	2990	

According to this, one metre length of overhead telegraph wire has the same capacity as a glass plate 1^{mm} thick, having a coated surface of 100 sq. mm. or 0·00001 sq. metre, and a conductor a German mile in length corresponds to a jar 1^{mm} thick, and having 7·7 sq. feet of internal coating.

Although the height of the wire above the surface of the ground was considerably greater than is usual with telegraph wires, the capacity of the latter will not be greater ; for, owing to the high buildings and trees which stand in its neighbourhood, the capacity of my wire is not inconsiderably increased, and because in general the inductive resistance is only a little increased with the greater distance from the ground, that is, in the ratio of the logarithms of twice the height, when it is great in proportion to the diameter of the wire. The inductive resistance between the wire and earth may be expressed according to Kirchhoff's formula for resistance by—

$$\frac{C \log \frac{2h}{r}}{2}$$

where h represents the distance of the wire from the earth, whence follows the correctness of the above assumption.

Of greater importance is the not inconsiderable charge of wires suspended in the open air, proved by examining the results of measuring the velocity of electricity. As later on I shall fully refer to the great retarding influence of the charge of cores on the production of the current at its further end, it only remains to remark here that the retardation of the current in cores is proportional to the square of the length of the wire. This already follows from the consideration that the time that is necessary in order to convey to each spot the quantity of electricity remaining behind in that part of the wire, and required for charging the same according to the electroscopic force coming to it according to Ohm's law, must be in direct proportion to the quantity of electricity, and inversely to the resistance to be overcome. As now with a wire of double length the quantity of electricity passing into the static condition, as also the mean of the resistance to be overcome, is double as great, it follows directly that the period of charge, after the completion of which the current can first appear at the end of the wire, must be four

times as great, and hence be in proportion to the square of the length of the wire. The measurements carried out on the velocity of propagation of electricity in wires have measured the sum of the losses of time depending on the charge, and on the velocity of the electricity, of which the first is in proportion to the square of the length, and the second to the simple length of the wire used. In this way the great differences between numerical estimates of the velocity are explained. The shorter and thinner the wires with which the experiments were made, the greater must they be. It is besides clear that the actual velocity of electricity must be much greater than the measured values, the correctness of the measurements being of course assumed. It further appears probable that the observed differences of time are only to be attributed to the charge of the wires.

As it is not possible to make conductors with which no charge occurs, the question of the velocity of propagation of the current deals only with an ideal case, the conditions of which cannot be realized. The only case in which the electrostatic induction in the neighbourhood of a wire is actually indefinitely small is when it is wound spirally; but then the electrostatic self-induction of the unequal electrical windings and their electrodynamic induction come into play, so that this case is also useless for measurements of velocity. Measurements of the velocity of electricity itself could hence only be carried into effect by measuring the retardation of the current at different distances from the battery, and from the series thus formed deducing the value for the time of charge and the velocity of the electricity. These considerations lead to the question what the so-called free electricity collected on the outer surface of the conductor actually consists of, and in what it is different from the charging or so-called "bound" electricity.

Faraday, as is known, has maintained the opinion that the so-called free electricity and bound or jar electricity are identical, and that in the first case the walls of the room form the outer coating of the jar.

The law of induction serves as a means of proving the correctness of this opinion. The inductive resistance dV of a very thin hollow sphere, the thickness of the wall of which is dx , and the radius x , is—

$$dV = \frac{dx}{4\pi x^2 k};$$

by k is understood the specific inductive capacity of the material of the hollow sphere. The total resistance of all the hollow spheres following one upon another is—

$$V = \frac{1}{4 \pi k} \int \frac{dx}{x^2}.$$

The integral taken between $x = k$ and $x = r$ gives—

$$V = \frac{1}{4 \pi K} \cdot \frac{R - r}{R \cdot r}.$$

Therefore—

$$Q = \frac{F}{V} = E \cdot 4 \pi k \frac{R \cdot r}{R - r}$$

and

$$\frac{Q}{E} = 4 \pi k \frac{R r}{R - r} \quad . \quad . \quad . \quad (5)$$

The expression $4 \pi k \frac{R r}{R - r}$ is what Riess has called the “strengthening coefficient” of the jar of the internal radius r and external radius R , for which with spherical jars the expression capacity can in general be used.

If R and r are very slightly different, and if one puts—

$$R - r = \delta,$$

and $4 \pi r^2 = F$,

the above equation becomes $\frac{Q}{E} = \frac{4 \pi r^2 k}{\delta} = \frac{F k}{\delta}$, which is identical with the formula explained by Poisson for the special case in which the thickness of the glass is very small as compared with the radius of least curvature.

A conductor placed in a room of ordinary dimensions may without any great error be considered, as regards the capacity of the condenser which it forms with the walls and floor of the room, as being in the centre of a hollow sphere of 3 metres radius. If the conductor is a sphere of 0.15 metre diameter, its capacity (for in this case k is equal to 1) is, according to equation 5,

$$\frac{Q}{E} = 4 \times 3.14 \times \frac{3 \times 0.15}{3 - 0.15} = 1.98.$$

The capacity of a glass sphere coated inside and outside of 0.15

metre internal diameter and 2^{mm} thickness of glass, is hence when $k = 2$ —

$$\frac{Q'}{E} = 4 \times 3.14 \times \frac{0.17 \times 0.15}{0.002} \times 2.$$

The ratio of the capacity of the two jars is consequently—

$$1 : 160.$$

Experiments which I made with a glass sphere covered internally with looking-glass amalgam, corresponds very exactly with this ratio. When freely suspended in the room, the sphere connected with a battery of 54 Daniell cells, without pole earthed, gave a deflection of 0.3°, and in water a deflection of 52°. Considering the uncertain determination of the mean distance of the walls of the room, and specially of the mean thickness of the glass, this agreement is greater than could be expected.

The electricity found on the outer surface of a body in the static condition can then always be considered as bound, latent, or produced by opposite electricity on neighbouring bodies, and a difference between both kinds of electricity is only to be found in the position of the observer whether inside or outside of the active dielectric.

If the charges or quantities of electricity Q and Q' of two spherical conductors of different sizes are compared, according to formula (5), then—

$$Q : Q' :: \frac{Rr}{R-r} : \frac{Rr'}{R-r'} = \frac{r}{R-r} : \frac{r'}{R-r'} \quad \dots \quad (6)$$

The quantity of electricity which is accumulated by means of equal electric forces on two spherical conductors of different sizes placed in equal spaces is hence not as their surfaces, but the small sphere contains more electricity per unit of surface than the large sphere, or, in other words :—

“The density of the electricity of the small sphere is greater than that of the large sphere.”

If F represents the surface of the spherical conductor, then the density $d = \frac{Q}{F}$.

Then $d : d' :: \frac{1}{(R-r)r} : \frac{1}{(R-r')r'}$ and when R is very great in proportion to r —

$$d : d' :: \frac{1}{r} : \frac{1}{r'} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

The densities of two spherical conductors in very large spaces, which are removed so far from one another that they exert no appreciable effect on one another, are inversely as the diameter of the spheres. This is to be explained in that the inductive resistance is mainly found in the layers of the dielectric lying next to the sphere. The smaller the radius of curvature of a surface, the more quickly the consecutive layers increase by expansion, and the resistance consequently diminishes. The inductive resistance reduced to the unit of surface is hence smaller with the small than with the large sphere, although the distance of the surface of the smaller from the boundary of the room is greater.

I have not yet been able to confirm by experiment whether the ratio of the density of the electricity on the surface of spherical conductors of different sizes given by equation (7) proves itself to be correct. I know of no experiments with frictional electricity by which it can be directly proved whether this proportion agrees with experiment. On the other hand, the expression for the charge of cores seems to indicate this. According to equation (4)—

$$Q = E \cdot 2 \cdot l \cdot \pi \cdot k \cdot \frac{1}{\log \frac{R}{r}} \cdot R$$

One can now suppose a wire of radius r , stretched across a room, as surrounded by a conducting cylindrical covering, and determine by this formula the quantity of electricity Q as a charge between the inner and outer cylinder. The density d of the electricity on the surface of the inner cylinder is then—

$$d = \frac{Q}{2 \pi r l},$$

i.e., the quantity of electricity divided by the surface. Consequently—

$$d = \frac{Q}{2 \pi r l} = \frac{E}{r \log \frac{R}{r}} \cdot \frac{R}{r} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

If R is now made equal to 5 feet = 60", and for r are consecutively substituted the radii 1", $\frac{1}{2}$ " and $\frac{1}{16}$ ", there results :—

$$\begin{aligned} d : d' : d'' &= \frac{1}{1. \log \frac{5 \times 12}{1}} : \frac{1}{\frac{1}{2} \log \frac{5 \times 12}{\frac{1}{2}}} : \frac{1}{\frac{1}{16} \log \frac{5 \times 12}{\frac{1}{16}}} \\ &= \frac{1}{\log 60} : \frac{2}{\log 120} : \frac{12}{\log 720} \\ &= 1.14 : 2 : 8.8 \end{aligned}$$

In the following table these values are combined with those which Coulomb gives for the density on wires of equal diameter electrified by contact with an 8" sphere :*—

TABLE XIII.

Diameter of Cylinder.	Density.		Differences.
	Observed by Coulomb.	Calculated.	
2"	1.3	1.14	+0.16
1	2	2	0
$\frac{1}{16}$	9	8.8	+0.2

The calculation agrees with observation better than was to be expected, when it is remembered that the substitution of a cylindrical and concentric room only depends on approximation, and neither the influence of the greater density of the ends of the wire nor yet the exhaustibility of the source of electricity used by Coulomb, *i.e.*, of an electrified 8" sphere, is taken into account.

According to equations (7) and (8), the density D of the sphere is to the density d of the replacing cylinder—

$$D : d = \frac{1}{r} : \frac{1}{r' \log \frac{R}{r}} = 1 : \frac{r}{r' \log \frac{R}{r}}.$$

If for r is substituted the radius of the 8-inch ball used by Coulomb, for r' cylinder radius 1, and for R , 60 as above, we get

* Riess, Lehrbuch der Electricität, Vol. I. p. 174.

$D : d = 1 : 0.977$; whilst Coulomb obtained the proportion $1 : 1.28$. It hence appears that R was chosen too great for Coulomb's experiments. By taking $R = 37$ inches as the most likely distance of his cylinder from the ground, there is obtained—

$$D : d = 1 : 1.12.$$

The smaller the radius r in the formula—

$$d = \frac{E}{r \log \frac{R}{r}}$$

the smaller the denominator of the fraction becomes, and hence the greater the density. If r is infinitely small, $d = \infty$. It hence follows that the density of the electricity of a perfect point becomes infinitely great.

These examples will suffice to show that Faraday's assumption that free static electricity, wherever and in whatever form it makes its appearance, is always in material antagonism through a dielectric with an equal quantity of opposite electricity, is not to all appearance opposed to facts, although it is to many very ingenious and hitherto generally acknowledged theories. From the evidence that free and induced electricity may be considered as identical, and that the distribution of electricity on the surfaces of conductors in certain important cases by electricity of higher potential is based on the necessary law of molecular distribution, the question arises whether the static distribution of electricity on the surface of the conductor cannot be conceived as exclusively a consequence of molecular induction.

I do not venture to answer this important question with an absolute affirmative, nor can I undertake to show that all variations in intensity dependent on the form of the conductors and their opposing influence are conditional on the law of molecular induction, as this would lead me far beyond what I must fix as the extreme limit of the present investigation ; yet I thought myself justified by the results brought together to consider this opinion as proved until the contrary is shown to be the case.

It is not to be assumed that two causes independent of one another affect the density of the electricity on the surface of bodies, of which each in certain cases not only explains, but even neces-

sarily causes the whole phenomenon. Hence if Faraday's opinion is correct that electrostatic induction is exclusively a molecular action, not a consequence of the direct attraction and repulsion of the electric fluids—and from the previous experiments it appears to me that there can hardly be any doubt on the subject—then it is also the cause of the difference of density of the electricity on the surfaces of conducting electrified bodies. Hence the force with which two electrified bodies, as experience shows, respectively attract or repel one another, cannot at the same time be the prime cause of the unequal distribution of electricity over the external surfaces of these bodies; or, in other words, attraction and repulsion is not a property of the electric fluids, but of electric matter.

Poisson founded his calculations on the density of electricity essentially on the condition considered as necessary by him, that the resultant of all attractive actions of the electricity on the surface of a body for any point chosen within it must be equal to zero, as otherwise a breaking up of the natural electricity of this point, and hence a disturbance of the assumed equilibrium must occur. If, however, the induction is exclusively a molecular action of the active dielectric, hardly any breaking up in the interior of the conductor by attractive action can occur. The first fundamental condition of Poisson's calculation thus gives way. Probably the second condition, "that the free outer surface of the electric surface must be a surface of equilibrium," can be deduced from the molecular law of induction, by which the contradiction between both theories would be removed.

A further consequence of Faraday's theory is the absolute distinction of the ideas "electric force or potential" and density of electricity. This distinction is most clearly shown in the view proved to be correct, "that the quantity of electricity of each element of surface" may be considered as produced "by an electric current of determined short duration through the dielectric considered as conducting throughout." The density or quantity of electricity per unit of surface corresponds, therefore, to the strength of current, not to the electromotive force of Ohm's law. In apparent opposition to this is the fact that the discharge and sparking distance of electricity, which we are accustomed to consider as direct potential phenomena, are manifestly related to the density. The discharge accompanied by evolution of light and heat, is,

however, evidently not a static, but a dynamic phenomenon, and is to be considered from this standpoint.

When two thin glass and mica plates are covered on one side with tinfoil, and the uncovered sides are laid on each other, so that there is a space filled with air between them of slight but equal thickness, a luminous appearance is produced, as is known, throughout the whole air space, when the condenser formed in this way is charged by means of a sufficiently powerful Leyden jar. This luminous appearance is repeated when the condenser is discharged. The air space is not illuminated when the jar is very slightly charged. It begins with a quite definite charge, and increases from this point with the increase of the charge of the jar.

From this phenomenon it is easy to draw the conclusion that the electric polarization of the molecules of a dielectric, which we must consider molecular induction to be, cannot exceed a determined maximum dependent on the nature and density of the body, and that a potential or polarization surplus is balanced or carried over by a sort of motion of a yet unknown character, which is accompanied with the evolution of light or heat or chemical action. If it is supposed that the whole inductive resistance of the glass was equal to that of the air space between the glass plates, and the electric force E was so chosen that the maximum of induction of the air was exactly reached, then if v represents the inductive resistance of the glass mass, $Q = \frac{E}{2v}$.

If now the active electric force E is doubled, the charge would be—

$$Q' = \frac{2E}{2v}$$

if the inductive maximum of the air space was not exceeded. But as, according to the supposition, this is already the case with half this charge, the charge may be supposed to be divided into two parts, of which one is equal to $\frac{E}{2v}$, and the other can be expressed by $\frac{E}{v}$ since the inductive resistance of the air is absent in the second part. The true charge of the condenser will consequently be—

$$Q'' = \frac{E}{2v} + \frac{E}{v} = \frac{3}{2} \cdot \frac{E}{v}$$

The discharge in an air-filled space must therefore be equal to $Q'' - Q' = \frac{E}{2v}$. The equivalent in work of this discharge in the interior of the dielectric must appear as light, heat, or alteration in the grouping of the molecules of the body, that is as chemical action. In the case in question, with the production of light and heat there is a simultaneous change of the oxygen of the air into ozone.*

Fig. 18.



If the inner glass surfaces separated by air were conductive, a quite equal discharge would appear, filling the whole air space between them only in the case when the surfaces are perfectly even and parallel and the density of the air perfectly uniform throughout. In other cases the discharge would begin first at the places which were nearest, or at which the electric density was greatest. As the heat accompanying the discharge rarifies the air acted upon, and its maximum of induction is thereby progressively diminished, the whole discharge must take effect where it has once begun. Instead of a general illumination, there will consequently be one restricted to a small space, an electric spark.

The discharge of a condenser, and consequently also of a conductor, may be analogously represented

* I have made use of this phenomenon for the construction of an apparatus which effects the conversion of oxygen into ozone by means of the induced current in the following manner:—

Two glass tubes of the very thinnest glass, of which one is closed at one end and is somewhat narrower than the other, are placed one inside the other, so that the annular space between them is of equal breadth throughout. Both tubes are fused together at one end, and the outer tube is provided with a short tubulure leading to the annular space, its other end being drawn out into a narrow tube. Fig. 18 shows this apparatus in vertical and horizontal section. A glass tube is thus formed with double walls, the space between which communicates with the outer air by means of two short tubes removed as far as possible from one another. If the inner and outer surfaces of the glass tubes are provided with a metallic coating, and the wires of the secondary coil of a powerful induction apparatus, fitted with a Wagner contact break, joined up to them, then the space between the two tubes begins to get luminous and the enclosed air is ozonized. By forcing air into the short tube the air can easily be changed, and in this way large quantities of air can be quickly ozonized.

by streaming or discharging sparks. A perfect point on a conductor must always stream out, as the density of the electricity at the point is infinitely great; consequently the induction maximum is exceeded in the nearest layers of air. The brush, that is the sphere of discharge, appears to extend until, in consequence of the enlargement of the extreme surfaces of the brush, the excess of the induction or polarization maximum of the air no longer exists.

If, on the other hand, a sphere connected to earth is approached to a charged conductor, the discharge must begin when, with the quickly increasing charge between the ball and the conductor, on the approach of the ball the limit of the maximum of polarization exceeds the point of greatest density of the surrounding air surfaces. Hence, that the discharging may begin in this the air is heated and rarefied, and its polarization maximum progressively diminished. The discharge must hence touch the distant portion of air, which is most strongly polarized, immediately become powerful, and in doing this remain limited to a less extended space.

If by this conception of the process of discharge, all the phenomena are not made sufficiently clear, still it shows that the fact that the discharge by sparks or brushes depends on the density and not on the electric force, is not opposed to the theory of molecular induction.

I have no intention of basing a general electrical theory on the theory of electrostatic molecular induction explained above, for I think that the experimental trials are not yet sufficiently complete. Finally, I will only remark thereon, that it is very likely that the seat of the electricity is removed from the conductors to the non-conductors surrounding them, and may be defined as an electrical polarization of the molecules of the latter. The conductors would then only be conceived as polarized spaces in an electrically polarized medium, with the property of being able to carry over the polarization in their medium from one point of their surface to another. If the specific inductive capacity of the conductor may be assumed as very great compared with that of the non-conductor, and proportional to its conductivity, its induction maximum on the other hand as infinitely small, then all conditions for the explanation of the phenomena of the electric current and also its attraction and repulsion appear to be fulfilled.

REMARKS ON WHEATSTONE'S AUTOMATIC WRITING TELEGRAPH.*

At the meeting of the 24th January, Mr. Wheatstone submitted to the Academy an automatic writing telegraph, with reference to which I propose to make the following remarks :—

The idea of transmitting telegraphic messages by means of a perforated strip of paper is in the first place very old and due to Mr. Bain. I remember that when I was in Paris, in the spring of 1850, Mr. Bain set an electro-chemical telegraph at work by means of a strip of perforated paper, at a meeting of the Academy at which I had the honour to be present.

Since that time, Mr. Halake and I have been much engaged with the application of the same method to Mr. Morse's telegraphs. Since the year 1855 we have supplied apparatus of this kind, called tachygraphs, on the line we have constructed from Warsaw to St. Petersburg. In the same year one of our tachygraphs was working at the great exhibition in the Champs Elysées, alongside of our apparatus intended for the simultaneous transmission of messages in opposite directions. The perforating apparatus with which our tachygraph was provided, used for perforating the strip of paper, only differed from that of Mr. Wheatstone just described by its greater simplicity. Long before Mr. Wheatstone's last communication to the Academy, we like him had used currents directed alternately in opposite directions for working our tachygraph, instead of currents and periods of rest alternately, and used strips of perforated paper to allow these currents to pass at the desired intervals. Lately, however, we have given up the use of these strips, having replaced them by movable type similar to printing type, and representing the different characters of the Morse alphabet. The operator prepares the message by arranging the type one after the other in the grooves, passed through the apparatus by clockwork. These types bring a commutator into play, which causes the current to pass in one direction or the other, according as the position of its reciprocating motion is determined by the passage of the types.

* *Comptes Rendus*, 1859, Vol. XLVIII. p. 468.

OUTLINE OF THE PRINCIPLES AND PRACTICE INVOLVED IN TESTING THE ELECTRICAL CONDITION OF SUBMARINE TELEGRAPH CABLES.

(*Paper read before the British Association at Oxford, 3rd July, 1860, by Dr. WERNER SIEMENS and C. WILLIAM SIEMENS.*)*

THE interruptions which have been hitherto only too frequently experienced in the more extensive lines of submarine telegraph cables, have been in almost all cases occasioned by a gradual decrease of insulation. When these cables have been repaired it has been generally found that the gutta-percha had become destroyed in parts by the electrolytic action of the current used for working the line, and especially in those places where the thickness of the insulating covering was less than the average, either in consequence of mechanical injury, or as was more frequently the case, owing to some cavity in the material, which had been squeezed in by the water, or in consequence of the wire occupying an eccentric position.

In places where the insulating covering of gutta-percha was of uniform and sufficient thickness, no decomposition or partial destruction of the material has been observed, even after the cable has been in use for years. The rapidity with which the work of destruction proceeds in faulty places is altogether dependent upon the intensity and duration of the current employed in working the line. Interruptions are produced proportionately more rapidly on long lines, owing to the greater resistance of the metallic conductor. Their progress can be retarded, but not altogether arrested by working with weak not alternating currents, and it may be taken for certain that so long as thin places are not altogether removed from the insulating gutta-percha covering of a submarine cable, its insulation will gradually but continually deteriorate.

It is therefore a matter of the utmost importance to prevent irregularities in the insulating covering as much as possible. The material employed should therefore be perfectly homogeneous; it should be applied to the wire in several layers closely adhering to

* From the Journal of the Austro-German Telegraph Union, 1860.

one another ; air-bubbles should be entirely avoided, and the concentricity of the entire covering should be ensured by the use of perfect machinery and the avoidance of any stoppages during the process of covering likely to cause any softening by heat of the different layers of the coating.

Great improvements have recently been made in the process of covering electric conductors with gutta-percha and intermediate layers of a composition known by the name of Chatterton's compound, as may be gathered from the fact that the cable prepared for the Rangoon-Singapore section proved to be ten times better insulated than that of the Red Sea line and to India before it was laid. This remarkable improvement was obtained by the great care taken by the Gutta-percha Company in the manufacture, combined with a system of continuous exact tests, with which we were entrusted by the British Government. These tests are specially directed to ascertain the specific conductivity of each mile of the covered wire ; all pieces of which the conductivity was below a certain fixed standard were rejected.

The extraordinary variation in the conductivity of the different kinds of commercial copper was made the subject of a very careful investigation by Dr. Mathiessen at the request of the British Government.

We found in practice that copper wire selected with the greatest care for telegraph conductors, varied as much as twenty per cent. in conductivity, and that the purest copper always conducted the best.

It is indispensable to test the conductivity of each separate mile of the insulated conductor, not only so as to reject faulty material, but also to obtain a complete record of the conductivity of each separate portion of the cable when completed, without which it is not afterwards possible by galvanic tests and calculations to determine with precision the position of faults.

The most difficult, and at the same time the most important tests, are those of the conductivity of each mile of the insulation,*

* To prevent any misunderstandings, we would remind the reader that gutta-percha, india-rubber, as well as all bodies which we are accustomed to call insulators, are not so in the absolute sense of the word, but that rather all bodies conduct electricity more or less, and the conductivity of so-called insulators is only infinitely small in comparison with that of metals. Experience has taught that even this slight conductivity of gutta-percha in the case

for it is not sufficient to find the larger faults or holes, but the places must also be ascertained where the wire lies eccentrically, and where there are bubbles and other little defects in the insulating material; all portions of the core must be rejected in which the insulating material is below the standard of conductivity.

For this purpose it was first of all necessary to determine the specific conductivity of the material used for the insulation of the wire, which experience has shown to be sufficiently uniform at constant temperature.

The effect of temperature on the conductivity of gutta-percha and other insulators has lately been thoroughly investigated by the Scientific Committee appointed by the British Government to enquire into the construction of Submarine Telegraph Cables. It suffices for our present purpose that as the result of these experiments, the conductivity of the insulating covering of the Rangoon-Singapore cable between the limits of 41° and 80° Fahrenheit (or 5° to 27° Centigrade), was found to increase nearly in the proportion of 1 to 7. The ratio of this enormous increase is however by no means constant; in the absence of exhaustive and reliable experimental results, we thought it advisable to carry out all our tests at a uniform temperature of 75° F. (24° C.). This comparatively high temperature has the advantage, that it is seldom actually exceeded after the cable is laid, and that the conductivity being about seven times as great at that temperature as at the winter temperature of 41° Fahr., the effect of small faults on the measuring instruments will also be proportionately more appreciable. In order to ensure uniform temperature as far as possible, the coils to be tested were placed for 24 hours in tanks of water maintained at a temperature of 75°; they were then removed and placed in a hermetically closed testing tank filled with water of the same temperature, and hydraulic pressure of at least 600 pounds to the square inch (about 42 atmospheres) applied, so that the water may penetrate into any existing cavities or cracks.

It is a remarkable fact which has been confirmed by observations on cables during submersion, that the conductivity of gutta-percha sensibly diminishes with hydrostatic pressure, but increases again

of long submarine cables, for which a very long and proportionately thin layer of this material is employed, becomes of importance, and consequently this mere trace of conduction is taken into account.

on removal of the pressure to something above the original amount. With slightly defective coils, however, the increase of the external pressure produces no increase but rather a decrease in the insulating power; we are thus enabled to discover faults which would not otherwise be perceptible. The ordinary methods of measuring the conductivity and insulation of conductors by the deflection of the needle of the ordinary galvanometer would be altogether insufficient for the purpose in question.

It was necessary to express the conductivity, both of the conductor and of the insulating covering, in simple figures representing units of resistance.

The unit of resistance we have adopted is the resistance of a column of mercury one metre long, and one square millimetre in section, taken at the temperature of 0° C. The advantages of this unit are explained by Dr. Werner Siemens in a paper published in Poggendorff's *Annalen*, Vol. CX., p. 1.* By expressing the resistance both of the wire and insulating material in terms of a definite unit of resistance, we not only gain the advantage of more accurate comparison between the results of different measurements, but afterwards, when the separate cores are united into a single cable, we obtain an excellent means of testing its electrical condition if we compare the total resistances of both the conductor and the insulating medium with the sum of the resistances previously obtained in testing the cores separately after allowance for change due to temperature.

But the principal advantage of this system of measurement consists in the possibility after the submersion of determining by measurement and calculation the position of any fault that has appeared in the cable as it lies on the sea bottom.

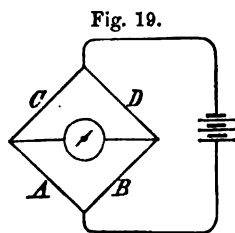
For convenience in practically carrying out this system of tests we constructed in the first place resistance coils of definite resistance which can be so combined that the total resistance can be varied at pleasure between the limits of 1 and 10,000 units.

By inserting these adjustable resistances in one branch of a Wheatstone bridge the resistance of the copper conductor as well as that of the insulating coating of a cable of considerable length can be easily determined. When however it comes to the measurement of resistances which lie beyond those limits a somewhat

different arrangement is given to the Wheatstone bridge by making its fixed branches also adjustable.

If A, B, C, and D are the four branches of a Wheatstone bridge, A C and B D represent the connections with the galvanometer, A B and C D those with the poles of the battery.

As is well known the resistances of the four branches are as $\frac{A}{B} = \frac{C}{D}$, when the current in the galvanometer branch = 0, and the needle of the instrument is therefore at rest. In the usual Wheatstone arrangement A equals B and the unknown resistance D is therefore measured directly by C. An adjustable resistance (Rheostat) inserted in place of C, containing units of resistance from 1 to 10,000, would only allow therefore of the measurement of resistances lying within these limits. When, however, A and B are arranged so as to be also adjustable, so that each of them can at pleasure have the value 10,100 or 1000, we can then measure resistances between 0.01 and a million units with an equal degree of accuracy. By means of this arrangement the resistance of copper wire of any length and of the insulating covering of long cables can be accurately determined within 0.2 per cent.



This method is not however applicable for testing short pieces of cable or longer cables insulated with better material, such as india-rubber or Wray's mixture,* because resistance coils of such various dimensions as would be necessary could not be employed without prejudice to their accuracy, principally because the greater battery power that would be required would heat the shorter branches considerably, and thus altering their resistance would cause important errors in the result.

It hence became necessary to employ another method for measuring the insulation resistance of short pieces of cable—say about a knot† in length. We employ in such cases a very sensitive

* Wray's mixture is believed to consist of india-rubber, shellac, and fine quartz powder. According to experiments made by Fairbairn in 1860 with different insulating materials proposed for submarine cables, this material excelled even india-rubber.

† Knots on the log line, unit for determining the velocity of ships by means of the log, = $\frac{1}{4}$ geographical mile.

sine galvanometer, or where the locality permits it a Weber's reflecting galvanometer with 40,000 convolutions and a magnetic mirror. The sensitiveness of this instrument can be varied by means of an adjusting magnet in the proportion of 1 : 100.

As the astatic condition of the needles of the sine galvanometer is subject to changes, the constant of the instrument should be repeatedly determined whilst testing.

The readings of the instrument in degrees are reduced to units of resistance by means of the formula—

$$R = \frac{\sin \phi'}{\sin \phi} \cdot n ;$$

where R is the insulation resistance to be measured, ϕ the deflection of the needle, $\sin \phi'$ the constant of the instrument, and n the number of cells in the battery. The proof of this formula is given in Appendix No. I.

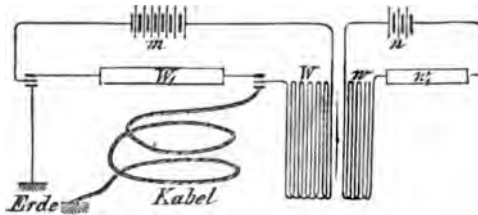
This method is only applicable, however, for the measurement of considerable resistance within certain narrow limits. The insulation resistance gradually diminishes as the manufacture of the cable progresses in proportion to its increase of length, and the instrument would soon be too sensitive. It could of course be made less sensitive in the same proportion, but in this way it would eventually not be sufficiently sensitive to measure correctly the resistance of the last core added to the cable. Therefore a means had to be contrived to maintain unaltered the original sensitiveness of the measuring instrument while the resistance gradually diminishes. For this purpose the coils of the sine galvanometer are surrounded by a second layer of comparatively few convolutions, through which the current of a small constant battery continually passes. The current for testing the insulation passes through the original coils of the instrument and is counteracted by the current in the outer coil passing in the opposite direction, which is so regulated by the insertion of resistances that it just neutralizes the influence of the other on the magnetic needle, which therefore continues in the zero position.

As the length of the cable increases, the resistance in the circuit of the outer coil must be diminished until the equilibrium of the needle is restored, and as the value of the variation of the resistance is known in units, this number has only to be multiplied by

the fixed ratio of the relative action of the two coils on the needle to obtain the desired result.

If W (Fig. 20) represents the resistance of the inner coils of the sine galvanometer, W' the resistance inserted in the inner circuit, and m the number of cells of the battery in this circuit ; further w the resistance of the outer (auxiliary) coil,* w' the resist-

Fig. 20.



ance and n the number of cells of the battery inserted in this circuit ; lastly K , a numerical co-efficient representing the constant proportion between the effect of each coil on the needle. Then we have—

$$\frac{n}{w+w'} K = \frac{m}{W+W'} \text{ or } K = \frac{m}{n} \cdot \frac{w+w'}{W+W'}$$

If now in place of W' the unknown resistance x of the cable is inserted in the circuit, and w' altered (to V) till the needle returns to the zero position, then if to make the statement quite general it is assumed that the number of cells in the batteries are no longer the same as before, but respectively M and N :—

$$\frac{M}{W+x} = K \cdot \frac{N}{w+V} \text{ or } x = \frac{1}{K} \cdot \frac{M}{N} \cdot (w+V) - W,$$

and by introducing the above value of K —

$$x = \frac{M}{N} \cdot \frac{n}{m} \cdot \frac{W+W'}{w+w'} (w+V) - W.$$

The chief advantage of this arrangement consists in the sensitiveness of the instrument remaining unchanged, since the whole

* In Fig. 20 the coils, for the sake of greater clearness, are shown not over, but near one another.

strength of the current passing through the insulation acts upon the needle, which is nevertheless always brought back to zero. In measuring the insulation resistance of short cables (where this resistance is very great), the resistance of the galvanometer coils W and w may be neglected in practice and the more simple formula $x = \frac{M}{N} \cdot \frac{V}{K}$ may be made use of. The coefficient K is independent of the sensitiveness of the instrument, and need only be determined once for all.

The tests are thus reduced to a very easy and simple method.

In order to calculate the insulation resistance of insulated wires from the specific conductivity of the material employed, and *vice versa*, we employ the formula—

$$W = \frac{\log e \cdot \frac{R}{r}}{2 \pi l \lambda}$$

The derivation of this formula is explained by Werner Siemens in Poggendorff's *Annalen*, an abstract of which is given in Appendix II.

This method serves to determine the insulation and conductor resistance of cables of all lengths, but does not comprise the tests necessary for the determination of its inductive capacity.

Recent experiments, which will be given further on, have proved that the specific inductive capacity of insulating materials is much more constant than their specific conductivity. The inductive capacity is besides independent of local defects in the insulating covering, and depends essentially on the geometrical form of the insulator. By measuring the inductive capacity of a given length of the cable, and comparing the result with the mean inductive capacity of the material employed, a means exists of determining with great certainty, whether the insulating material is laid equally around the conducting wire, throughout its whole length, or whether the wire is eccentric in places. The knowledge of the inductive capacity of a cable is besides absolutely necessary in order to ascertain the position of the fault should a break occur in the conductor, when the ruptured end remains insulated.

According to Faraday's idea, the inductive action is communicated from the inner coating of a Leyden jar to the outer from

atom to atom through the dielectric. In our case the Leyden jar is represented by the cable, the inner coating of which is formed by the surface of the copper wire, and the outer by the water.

The laws of the motion of heat and electricity in conductors are hence directly applicable to electric induction, and so the inductive capacity may be expressed by the product of the conductivity multiplied by a constant factor, whose value depends on the nature of the insulating material.

Starting from this point of view the inductive capacity of any insulated wire will be represented by the formula—

$$K = \frac{2 \pi \cdot l \cdot J}{\log \epsilon \frac{R}{r}} \cdot C,$$

in which the specific conductivity λ of the previous formula is replaced by the specific inductive capacity J . The unit of inductive capacity is that of a Leyden jar, the coatings of which have a surface of a unit of area placed at a unit distance from each other.

Professor W. Thomson has obtained, in a direct and very elegant manner, a formula which only differs from the above in the value of the constant, proving that he has started from a different unit; Werner Siemens' derivation of the formula is completely explained in the paper in Poggendorff's *Annalen*, Vol. CII. already mentioned.

In its application to cylindrical jars or to cables the formula takes the simple form—

$$K = \frac{J \cdot C}{\log \epsilon \frac{R}{r}}.$$

In our experiments the inductive capacity of the Leyden jar was measured by the deflection of a galvanometer needle. If the deflection of the needle is caused by a current of very short duration, the quantity of electricity passing through the galvanometer is equal to—

$$K = \frac{1}{E} \sin \frac{1}{2} A.$$

Practically it is found very difficult to read off with sufficient

accuracy the sudden deflection of a needle, and we employ therefore an instrument enabling us to obtain a rapid succession of charging and discharging currents, which in passing through the galvanometer produce a steady deflection of the needle, capable of being read easily and with great accuracy. The value of these deflections can be calculated by the following formula :—

If A is the angle through which the divided circle of the sine galvanometer has to be turned to bring the needle back to zero. C the number of charges or discharges per second E , the electromotive force of the battery, then we have—

$$K = \frac{1}{E} \cdot C \cdot \sin A ; *$$

or if K' is the inductive capacity of the Leyden jar chosen as the unit of comparison, and A' the corresponding deflection of the needle, we have—

$$K = K' \cdot \frac{\sin A}{\sin A'}$$

By permission of the British Government we have been able to test by this formula the experimental cable prepared to their instructions.

The results of these measurements, which are tabulated in Appendix III., prove satisfactorily the accuracy of the method employed. They also prove that the formula made use of for the specific inductive capacity, which Professor Thomson and Werner Siemens arrived at in entirely different ways, may be thoroughly relied upon in practice.

The specific inductive capacity of all gutta-percha covered wires is found to be nearly the same and quite independent of their specific conductivity, whilst india-rubber and its compounds have a much smaller inductive capacity. If the specific inductive capacity of gutta-percha be taken as the unit, that of india-rubber is only 0·7, and of Wray's mixture 0·8.

We have still to mention the methods frequently employed of ascertaining by means of a sensitive electrometer the diminution

* In this case the amount of the charge is measured by a constant deflection of the galvanometer needle, and is hence represented by $\sin A$, whilst above, where the first deflection is used, it is equal to $\sin \frac{1}{2} A$.

of the potential in a cable when left to itself after being highly charged.

Let E represent the potential of a battery connected to the cable, as observed on a sine galvanometer; y the potential remaining after the lapse of time t ; K the specific inductive capacity, and w the resistance of the insulator; then during the interval of time from t to $t + dt$, in which the potential diminishes by dy , there will be according to Ohm's law a discharge current $\frac{y}{w}$. Hence we obtain the equations:—

$$K \cdot dy = \frac{y}{W} \cdot dt, \quad \frac{dy}{y} = \frac{dt}{K W} \quad \text{and therefore} \quad C - \log e y = \frac{t}{K W}$$

Further, as for $t = 0$, $y = E$,

the integration constant $C = \log e E$, it follows that—

$$\log e \frac{E}{y} = \frac{t}{K W} \quad \text{or} \quad \frac{E}{y} = e^{\frac{t}{K \cdot W}} \quad \text{and} \quad y = E \cdot e^{-\frac{t}{K \cdot W}}$$

In a normal cable—

$$K = \frac{2 l \pi \cdot J}{\log e \frac{R}{r}} \quad \text{and} \quad W = \frac{\log e \frac{R}{r}}{2 \pi \cdot l \cdot \lambda} \quad \text{or} \quad K \cdot W = \frac{J}{\lambda}$$

and hence $\log e \frac{E}{y} = \frac{t \lambda}{J}$ or $\lambda = \frac{J}{t} \cdot \log e \frac{E}{y}$, which gives at last the proportion—

$$\lambda : J = \log e \frac{E}{y} : t$$

This method is well adapted for determining the specific resistance of insulating materials, and for comparing the insulation of two similar cables, even when there is no instrument at hand suitable for exact measurement. It is sufficient to observe the time in which the original potential falls to a given amount. As then the fraction $\frac{E}{y}$, although its value is unknown, is always the same, it follows from the above formula that—

$$\frac{\lambda \cdot t}{J} = \frac{\lambda' t'}{J} \quad \text{and} \quad \frac{\lambda}{\lambda'} = \frac{t'}{t}$$

where λ and t represent specific conductivities and observed times in both experiments.

This result is independent of the wire lying absolutely in the centre of the insulating covering or its being eccentric in places. This method of observation is therefore well suited for determining the specific resistance of the insulating material ; but as it is also necessary to ascertain whether the wire is concentric in the covering throughout the whole length of the cable this method cannot be exclusively employed. Besides, this process requires much time with well-insulated cables. Another objection to the exclusive application of this method arises from the circumstance that small faults in long cables easily escape observation, as the loss of potential through such faults is very small as compared with the whole charge.

We therefore prefer to determine the loss of potential not by the electrometer, but by measuring by means of a galvanometer the charge a , and after the lapse of a minute the discharge b . We have then the loss in quantity or potential during a minute—

$$L = 1 - \frac{b}{a}.$$

In order to combine this formula with the system just explained, it is only necessary to remember that $\frac{b}{a} = \frac{\eta}{E}$. If the cable is tested from the earliest stage of its manufacture in lengths of a knot, then during joining and covering, and finally during submersion, these tests must strictly control each other ; they must, therefore, be systematically recorded. The chief care during submersion should be directed to detect at once the slightest change in the insulation, so that the paying-out machinery may be instantly stopped. It sometimes happens, however, that a fault does not appear immediately on submersion. When, therefore, a fault appears, it is necessary to calculate its exact position before taking other steps to remove it. For this purpose the cable must be tested from both ends, that is, from the ship and from shore, as the determination from one end gives only the maximum distance of the fault.

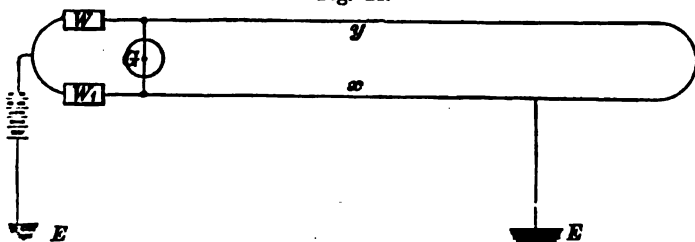
In paying out the cable, the following system of tests is applied :—

A clockwork placed at the shore station connects the cable consecutively to earth, to the pole of a battery, and then insulates it for a short time.

On the ship a resistance bridge is constantly connected with the line. By maintaining a balance on the Wheatstone bridge, the electrician in charge is able to determine alternately the insulation and conductor resistances ; the latter of which is conclusive as to the continuity of the conductor.

The operator on shore also observes these two data, and communicates them by telegraph to the ship. If these four tests differ materially, the existence of a fault is thereby indicated, the position of which can be calculated from the observed data. This method of observing the condition of the cable, although fatiguing to the electricians employed, has been found to answer perfectly in laying the Indian cables. During the paying out of the Aden-Kurrachee section by Messrs. R. S. Newall and Co., we were able

Fig. 21.



by this means to observe faults on five different occasions, which it was then possible to remove without stopping.

Our method for determining the position of a fault is the following :—

First, when both ends of the cable are available, let x and y (Fig. 21) represent the distance of the fault from these two ends ; $l = x + y$ the whole length of the cable ; G a galvanometer ; W and W' two resistance boxes. When W and W' are so adjusted that the galvanometer needle remains at rest, the position of the fault will be given by the formula—

$$x = \frac{l \cdot W}{W + W'} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This is essentially the same method as was published by C. Siemens in the Journal of the Austro-German Telegraph Society in 1858, p. 13, having been used by us with perfect success since 1848.

Secondly, in dealing with a single submerged cable, this method is no longer applicable. If c is the resistance of the whole length of the cable, x and y the resistances from the fault to the two ends, Z the resistance of the fault itself; a and b , and a_1 and b_1 , resistances measured from the two ends, whilst the other ends are respectively insulated and connected to earth. Then, in accordance with Ohm's law, we have the following equations:—

$$c = x + y, \quad a_1 = x + Z, \quad b_1 = y + Z.$$

$$a = x + \frac{Zy}{Z+x} \text{ and } b = y + \frac{Zx}{Z+y}.$$

By the elimination of y and Z , the following expressions are obtained :—

$$x = \frac{a_1 - b_1}{2} + \frac{c}{2} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$x = a \cdot \frac{c - b}{a - b} \cdot \left\{ 1 - \sqrt{\frac{b}{a} \cdot \frac{c - a}{c - b}} \right\} \quad . \quad . \quad . \quad (3)$$

$$\frac{x}{y} = \sqrt{\frac{a}{b} \cdot \frac{c-a}{c-b}} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$x = a - \sqrt{(a_1 - a)(c - a)} \quad . \quad . \quad . \quad (5)$$

If the cable was not perfectly insulated before the appearance of the fault in question, the measurements a and b and a' and b' taken before the appearance of the new fault serve as a means to determine approximately the resistance γ of the previous fault of insulation. With the help of this value, and the final measurements a_2 and b_2 , taken after the fault appeared when the other ends are insulated, the position of the fault is given in the following manner :—

$$x = a_1 - \gamma \sqrt{\frac{a_1 - a_2}{b_1 - b_2}} \quad . \quad . \quad . \quad . \quad (6)$$

In all these measurements the battery power must be regulated

to maintain the polarization of the faulty place uniform. For this purpose the position of the fault is, first of all, approximately ascertained by means of preliminary measurements, and then for the final measurement the number of cells in the battery is so regulated that the current passing to earth from the one or other side through the fault has always nearly the same strength; it is necessary not to make the observation until the polarization has reached its maximum. We attach considerable importance to the last formula, which alone enables us to localize new faults in old defective cables if only its previous state of insulation is known. This knowledge is unfortunately wanting as regards almost all cables hitherto laid. In the case of the Rangoon Singapore line we propose to provide each station with a complete testing apparatus, and to arrange for daily tests to be made of the insulation and conductivity of each section of the cable when laid. Records of these observations should be sent daily to the engineer in charge.

Owing to the easy destructibility of gutta-percha various experiments have been made to replace this material in submarine cables by india-rubber, sometimes pure and sometimes mixed with other materials, experiments which owing to the high insulating power and the slight inductive capacity of india-rubber, and above all its great homogeneity and slight sensitiveness to heat, gave promise of great results as regards the durability of telegraph cables.

The great difficulty has hitherto consisted in working india-rubber so as to obtain a uniform and perfect coating over the conductor without damaging the material itself. We have sought to overcome this difficulty by the construction of a machine which was exhibited at the British Association. Important work is also being done in this connection elsewhere.

The principal object of this communication is to show that, although frequent accidents have befallen submarine telegraph lines owing to insufficient experience and want of care during their manufacture to guard against defects, the experience gained has not been lost; and that, in making use of the increased knowledge of the subject, more perfect results could not fail to follow.

The British Government, in promoting these experiments, has encouraged and directed individual effort, proving that England

fully appreciates the importance of submarine telegraph lines, and is prepared to call them into existence.

APPENDIX I.

INSULATION RESISTANCE OF SHORT CABLES.

If one pole of a battery of n cells is connected with the cable while the other is to earth, then, if ϕ represents the angle through which the sine galvanometer must be turned to bring the needle back to zero, the following equation results—

$$\sin \phi = \frac{n E}{x + w_1}$$

in which E is the electromotive force of one cell, w_1 the resistance of the galvanometer, and x the unknown resistance of the cable.

To obtain the actual value of the insulation resistance, a known resistance u of about 10,000 units is inserted in the circuit instead of the cable, the sensitiveness of the instrument is diminished by a shunt having a resistance W_1 (about $\frac{1}{100}$) and the number of cells reduced to one. There is then obtained for the whole current J in the circuit the expression—

$$J = \frac{E}{u + \frac{W_1 W_2}{W_1 + W_2}}$$

and for the branch current i_1 passing through the galvanometer—

$$i_1 = \sin \phi_1 = \frac{W_2}{W_1 + W_2} \cdot \frac{E}{u + \frac{W_1 W_2}{W_1 + W_2}}$$

or since $W_1 = 99 W_2$:

$$i_1 = \sin \phi_1 = \frac{E}{100} \cdot \frac{1}{u + \frac{99}{100} W}$$

$\frac{99}{100} W_2$ being with our instruments equal to 70 units, if we take this value and choose the known inserted resistance as 9930 units instead of 10,000, we have—

$$\sin \phi_1 = \frac{E}{100} \cdot \frac{1}{10,000} = \frac{E}{1,000,000}$$

If we eliminate E between this and the first equation, and neglect the resistance W_1 of the galvanometer in comparison with x , we arrive finally at the expression—

$$x = 1,000,000 n \frac{\sin \phi_1}{\sin \phi}.$$

APPENDIX II.

THE SPECIFIC RESISTANCE OF INSULATING MATERIALS.

DEDUCTION OF THE NECESSARY FORMULA.

WERNER SIEMENS arrived in a simple way at the same formula which Professor Thomson arrived at in another way by calculation.

If dx represents the thickness of a differential cylinder at the distance x from the longitudinal axis, its resistance will be—

$$dw = \frac{dx}{2\pi \lambda \cdot l x},$$

and the total resistance—

$$W = \frac{1}{2\pi l \lambda} \int_r^R \frac{dx}{x} = \frac{\log \epsilon \frac{R}{r}}{2\pi l \lambda}$$

APPENDIX III.

RESULTS OF THE MEASUREMENTS OF SPECIFIC INDUCTIVE CAPACITY.

In the following table are collected the results of the specific inductive capacity.

The following is descriptive of the contents of the several columns of the table :—

The second column gives a description of the cable as regards insulating material and the test-mark of the cable ; the 3rd its length ; in the 4th, 5th, and 6th columns are found the outer and

inner radius of the insulating covering and the ratio $\frac{R}{r}$; the 7th column gives the temperature of the water in which the cable was placed; 8th, the number of cells employed in the test; the 9th contains the angle ϕ through which the sine galvanometer had to be turned to bring back the needle to the zero position; the 10th the constant of the instrument; the 11th the measured charge in terms of the unit of length and one cell; in the 12th are found, lastly, the calculated specific inductive capacities of the different substances, the mean inductive capacity of gutta-percha being taken as 1.

No.	CABLE.		Radius.			F. Temperature.	No. of Elements (potentials).	Deflection ϕ .	Constant of Instrument.	Charge $\frac{\sin \phi}{n \cdot l}$.	Spec. Induct. Coeff. $\log \epsilon \frac{R}{r} \cdot C$ Gutta-percha = 1.
	Material and Marks.	Length. m. Engl. Miles. y. Engl. Yards.	Internal r.	External R.	$\frac{R}{r}$						
1	Gutta-percha A	$\frac{1}{2}$ m.	4	16	4	51	7	20.8	1	9.92	1.06
2	" B	$\frac{1}{2}$ m.	72	5	25	1.7	9.82	1.07
3	" A+B	1 m.	51	7	20.1	1	9.9	1.06
4	" A	$\frac{1}{2}$ m.	7	48.4	1	...	9.8	1.05
5	" B	$\frac{1}{2}$ m.	5	29.1	1	...	9.7	1.04
6	" A+B	1 m.	3	16.5	1	...	9.5	1.02
7	" A	$\frac{1}{2}$ m.	2	14	7	7	14.5	1	...	7.14	1.07
8	" B	$\frac{1}{2}$ m.	14.2	7.0	1.05
9	" C	$\frac{1}{2}$ m.	28.5	6.8	1.048
10	" D	$\frac{1}{2}$ m.	10	2.5	...	3	12.3	14.2	1
11	" A+B	1 m.	8	12.2	14.2	1
12	" A+B+C	$\frac{1}{2}$ m.	8	12.5	14.4	1.002
13	" A+B+C+D	2 m.	3	15.4	1.7	...	14.5	1.003
14	" A	1 m.	2	8	4	2	15.4	1.7	...	14.5	1.003
15	" B	1 m.	51	8	25.6	1	14.4	1.0
16	" A+B	2 m.	40.3	14.4	1.0
17	" A	1 m.	61.0	14.38	0.999
18	" B	1 m.	10.2	1	...	9.3	1
19	" A+B	2 m.	72	2	17.8	1.7	9.4	1.2
20	" A	1 m.	1	7	7	51	3	16.6	1	9.5	1.208
21	" B	1 m.	72	...	31.1	1.7	9.4	0.99
22	" A+B	2 m.	92	...	25.9	1.45	9.4	0.99
23	" A	1 m.	51	...	34.6	1	9.38	1.003
24	" B	1 m.	11.4	1	6.58	0.99
25	" A+B	2 m.	92	...	20.6	1.7	6.4	0.99
26	" A	1 m.	17.7	1.45	6.5	1.2
27	" B	1 m.	51	...	11.4	1	6.58	0.99
28	" A+B	2 m.	72	5	31.8	1.7	6.49	1
29	" A	1 m.	51	8	22.8	1	6.47	0.98
30	" B	1 m.	1	4	4	16	1	9.18	0.99
31	" A+B	2 m.	72	2	19.4	1.7	9.2	1.02
32	" A	1 m.	51	3	16	1	9.18	0.99
33	" B	1 m.	72	5	28.4	1.7	9.2	0.99
34	" A+B	2 m.	92	8	24.6	1.45	9.10	0.98
35	" A	1 m.	18	13	...	51	3	32.8	1	9.03	0.97
36	" B	$\frac{1}{2}$ m.	4.8	...	5.6	1.12
37	" A+B	$\frac{1}{2}$ m.	72	5	16.2	1.7	5.5	1.09
38	" A	$\frac{1}{2}$ m.	92	5	14.9	1.45	5.5	1.09
39	" B	$\frac{1}{2}$ m.	51	3	4.8	1	5.6	1.12
40	" C	$\frac{1}{2}$ m.	4.8	1	5.6	1.12
41	" D	$\frac{1}{2}$ m.	4.8	...	5.6	1.12

No.	CABLE.		Radius.			Temperature F.	No. of Elements (potential)	Deflection ϕ .	Constant of Instrument.	Charge $\frac{\sin \phi}{\pi \cdot l}$.	Spec. Induct. Coeff. $\frac{R}{2\pi l} \cdot C$ Gutta- percha=1.
	Material and Marks.	Length. m. Engl. Miles. y. Engl. Yards.	Internal r.	External R.	R						
27	Gutta-percha A+B	1 m.	0.2	...	5.3	1.10
28	" A+B+C	1 1/2 m.	13	...	5.2	1
29	" A+B+C+D	2 m.	17.9	...	5.12	1.1
30	Harder	450 y.	2	6	8	5	...	13	1.24
31	Wray A	1 m.	1	7	7	5	...	5.17	0.77
	" "	"	72	8	16.1	1.7	5.2	0.625
	" "	"	92	8	...	1.45	5.4	0.96
	" "	"	51	...	8.8	1	5.1	0.77
32	Silver B	1 m.	1	6.2	6.2	51	5	13.9	1	4.8	0.67
	" "	"	72	8	14.5	1.7	4.9	0.686
	" "	"	92	...	12.8	1.45	5	0.705
	" "	"	51	5	14.2	1	4.8	0.68
	" "	"	72	...	18.1	1.7	4.8	0.619
33	" C	1 m.	1	7.2	7.2	51	...	12.8	1	4.45	0.66
	" "	"	72	8	12.9	1.7	4.3	0.513
	" "	"	92	8	11.6	1.45	4.7	0.666
	" "	"	51	5	13	1	4.5	0.66
34	" C+B	2 m.	27.6	1	4.65	0.66
	" "	"	27.6	...	4.65	0.66
35	Hall & Wells A	1 m.	1	4	4	51	5	34.6	1	11.3	1.21
	" "	"	72	1 1/2	18.6	1.6	11.6	1.38
	" "	"	92	8	32.9	1.6	11.6	1.32
36	" B	1 m.	51	5	35.2	1	11.5	1.21
	" "	"	25	1	8.45	0.92
	" "	"	72	8	26.8	1.7	8.5	0.91
	" "	"	92	8	23.8	1.45	8.5	0.967
	" "	"	51	5	24.2	1	8.2	0.92
37	Hughes A	1 m.	1	6	6	51	5	16.4	1	5.66	0.83
	" "	"	72	8	17.8	1.7	5.67	0.84
	" "	"	92	8	16.2	1.45	5.63	0.88
	" "	"	51	5	16.6	1	5.66	0.83
38	Many coatings A	1 m.	1	7	7	51	5	19.8	1	6.61	1.0
	" "	"	1	6.61	1.0
	" "	"	72	8	20.2	1.7	6.62	1.08
	" "	"	92	...	17.7	1.45	6.62	1.04
39	Gut. per. spec. A	1 m.	1	4	4	51	5	21.2	1	7.23	0.74
	" "	"	72	8	20.6	1.7	7.23	0.74
	" "	"	92	...	18.1	1.45	7.8	0.766
	" "	"	51	5	21.4	1	7.29	0.445
40	" B	1 m.	20.2	1	6.9	0.74
	" "	"	72	8	20.9	1.7	6.98	0.754
	" "	"	92	...	18.4	1.45	6.9	0.75
	" "	"	51	5	20.8	1	6.95	0.74
41	" C	1 m.	1	7	7	51	...	90.4	0.74
42	Radcliff A	1 m.	1	7	7	51	5	21.2	1.7	7.23	1.09
	" "	"	72	8	22.1	1.45	7.8	1.1
	" "	"	92	8	18.8	1	7.3	1.1
	" "	"	51	5	21.4	1	7.23	1.1
43	" B	1 m.	1	7	7	51	5	22.4	1	7.62	1.10
44	" "	"	72	8	22.9	1.7	7.68	1.15
	" "	"	92	...	19.4	1.45	7.63	1.15
	" "	"	51	5	22	1	7.59	1.14
45	Godefroy	1 m.	1	4	4	51	...	30.6	1	10.18	1.08
	" "	"	51	5	31	1	10.3	1.1
	" "	"	92	8	28.8	1.45	10.6	1.12
46	Gutta-percha	1 m.	51	5	38.6	1	9.57	1.1
	" "	"	31.0	1	9.57	1.08
47	Siemens	500 y.	51	5	8.1	1.7	7.3	0.77
	" "	390 y.	72	5	6.5	1.45	7.4	0.78

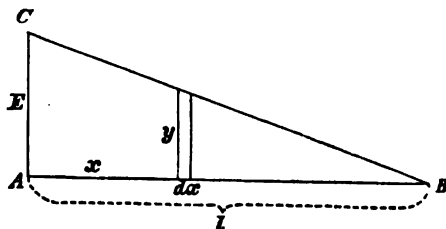
APPENDIX IV.

DISTRIBUTION OF THE CHARGE ALONG THE LENGTH OF THE CABLE.

LET AB (Fig. 22) represent a given length (l) of uncoiled cable, the end of which, B , is in connection with the earth, and A C the E. M. F. of a battery, one pole of which is connected with A and the other with the earth. Supposing the cable to be of equal section and conductivity throughout, then according to Ohm's law, the E. M. F. existing at the different points of the cable is represented by the curve BC .

In the year 1849, Werner Siemens showed in Poggendorff's

Fig. 22.



Annalen that when a current is sent through a submerged or underground cable a portion of the electricity is retained as a charge along the whole surface, being distributed proportionately to the potential at each point.

The potential of the electricity of an element of length dx at the distance x from A being represented by y , and the quantity of electricity with which the surface of the cylinder dx is charged by dq —

$$dq = K y = y \cdot \frac{2 J \cdot \pi dx}{\log e \frac{R}{r}}.$$

This quantity of electricity dq has to overcome the resistance of the portion x on its way to dx .

The resulting current requires the time dt to produce the quantity dq ; we have therefore the equation—

$$dq = \frac{E \cdot dt}{\frac{x}{r^2 \cdot \pi l}} = - \frac{E r^2 \pi \lambda}{x} \cdot dt.$$

By equating these values of dq we obtain the differential equation—

$$y \cdot \frac{2 J \cdot \pi \cdot dx}{\log \epsilon \frac{R}{r}} = \frac{E r^2 \pi \lambda}{x} \cdot dt.$$

If we substitute for y in the differential equation, its value obtained from the proportion—

$$E : y = l : l - x, \text{ viz. } y = E \left(\frac{l - x}{l} \right)$$

we obtain—

$$dt = \frac{2 J}{l \cdot r^2 \lambda \log \epsilon \frac{R}{r}} \cdot x (l - x) dx,$$

$$\text{and thence } t = \frac{2 J}{l \cdot r^2 \lambda \cdot \log \epsilon \frac{R}{r}} \cdot \int_0^l x (l - x) dx$$

$$= \frac{2 J}{l \cdot r^2 \lambda \cdot \log \epsilon \frac{R}{r}} \cdot \left(\frac{l^3}{2} - \frac{l^3}{3} \right)$$

$$\text{and finally } t = \frac{J l^2}{8 r^2 \lambda \cdot \log \epsilon \frac{R}{r}}$$

APPENDIX V.

MEASUREMENT OF THE RESISTANCE OF THE INSULATING LAYER.

THE following table contains the results of the measurements of the specific resistance of the insulating material. The figures are derived by means of the formulæ given in Appendices II. and III., from the deflections obtained by the use of a certain number of battery cells; 18 noughts are omitted in the constants, therefore the same are divided by 10^{18} .

These numbers therefore represent the specific resistance compared with mercury in trillions of units. Certain variations may be explained partly by the action of very powerful battery currents, with which the cable was worked during manufacture, and partly

Material.	R.	r.	32° Fahr.			52° Fahr.			72° Fahr.			92° Fahr.		
			No. of Elements.	Deflection.	Specific Resistance.	No. of Elements.	Deflection.	Specific Resistance.	No. of Elements.	Deflection.	Specific Resistance.	No. of Elements.	Deflection.	Specific Resistance.
Gutta-percha.	13	1	256	5.6	0.916	64	16	0.5	64	19.2	0.325
"	3.6	1.45	...	12.5	0.637	...	16.9	0.368
"	7	1	2	2.8	4.40	128	2.6	2.63	...	21.9	0.969	32	20.2	0.407
"	4	1	36.7	0.85	16	26.5	0.221
"	8	2	64	2.4	1.89	...	70.8	0.538	...	21.6	0.264
"	10	4	256	13.2	1.09	...	30.6	0.996	...	11	0.393
"	256	6.4	2.23	...	21.9	1.03	...	9.6	0.447
Silver.	256	1.4	48.9	512	4.4	41.5	512	4.6	31.0
"	7.2	1	1.2	52.4	...	3.4	49.0	...	5.3	24.3
Wray.	6.2	1	2.6	23.6	...	6.4	26.0	...	15.7	38.4
Siemens.	7	1	0.5	38.4	...	1.1	49.55	...	0.9	38.4
G. P. spec.	4	1	128	21.4	2.25	...	21.1	8.25
"	4	1	4.7	9.44	64	6	4.86	32	1.3	8.38
"	4	1	512	5.5	22.9	8.6	1.55
Hughes.	7	1	64	16.7	1.05	8	18.9	0.153	...	11.6	0.191
Many coatings.	6	1	256	8	7.86	256	21.4	3.98	32	12.8	0.696
Hall and Wells.	7	1	128	128	8.3	5.53	256	27.5	3.11
Radcliff.	7	1	2	15.9	0.041	2	11.2	0.057
"	16	8	0.48	16	15.0	0.350	16	8.6	9.642
Godefroy	46	14.9	0.496	16	13.2	0.482

by the circumstance that time enough was not allowed for obtaining a perfectly uniform temperature throughout the cable and various other circumstances.

Setting aside these irregularities, the great difference between the resistance co-efficients of gutta-percha, india-rubber, and Wray's compound is nevertheless very clearly set forth.

DESCRIPTION OF UNUSUALLY STRONG ELECTRICAL PHENOMENA ON THE CHEOPS PYRAMID NEAR CAIRO DURING THE BLOWING OF THE CHAMSINS.*

WHEN on the 14th of April last year, I ascended the Cheops pyramid, with the engineers employed in assisting me in laying the telegraph line in the Red Sea, we had an opportunity of observing an unusually strong electrical phenomenon on its summit.

When we left Cairo, early in the morning, the sky was as bright and clear as usual, and there was hardly a breath of wind stirring. A light, pale red tint in the south-west horizon appeared, however, to alarm my donkey-driver, who was continually pointing in that direction, and it appeared to be the reason of his driving my beast on very energetically.

At half-past nine we arrived at the foot of the pyramid, and about twenty minutes later we found ourselves on its summit, less as the result of our own exertions than owing to the powerful impulses which each of us in turn received from three powerful Arabs, who flung us from stage to stage like bales of goods. Arrived at the summit we felt a sharp, cold wind blowing. The reddening of the south-western horizon was changed to a colourless clouding over right up to the zenith, so that instead of obtaining the view we hoped of the valley of the Nile and the town of Cairo, we could only observe near-lying objects in feeble outline. We laid down behind the blocks of stone, which were scattered about on the flat summit of this pyramid, to rest ourselves after the

* Poggendorff's Ann. der Phy. u. Chem., 1860, Vol. CIX. p. 355.

exertion of our involuntary race, and to protect ourselves from the cold wind, which was continually increasing in strength.

It was interesting to observe the sand of the desert, which covered the plain with an opaque yellow veil, continually rising with whirling motion higher up the pyramid. When it had arrived at the highest step we noticed a whistling noise which I ascribed to the increasing violence of the wind. The Arabs who were squatted around us on the nearest steps, sprang up suddenly with the cry "Chamsin," and held up their fore-finger in the air. There was now a peculiar whistling noise to be heard, similar to that of singing water. We thought at first that the Arabs were uttering this sound, but I soon satisfied myself that it also took place when I stood upon the highest point of the pyramid, and held up my own fore-finger in the air. There was also a slight, hardly perceptible, pricking observable on the skin of the finger which was opposed to the wind. I could only explain this fact, observed by all of us, as an electrical phenomenon, and such it proved to be. When I held up a full bottle of wine, the top of which was covered with tinfoil, I heard the same singing sound as when the finger was held up. At the same time little sparks sprang continually from the label to my hand, and when I touched the head of the bottle with my other hand I received a strong electric shock, whilst a bright spark sprang from the metal-top of the bottle to my hand. It is clear that the liquid inside the bottle, brought into metallic connection with the metallic covering of the head of the bottle through the damp cork, formed the inner coating of a Leyden jar, whilst the label and hand formed the outer connected to earth. An uncorked bottle was charged in a similar way, especially when the neck was directed towards the wind, as Dr. Esselbach perceived by a violent shock which he got when he placed it to his mouth. When I had completed the outer coating of my bottle by wrapping it in damp paper from our provision basket, the charge was so strong that I could make use of it as a very powerful weapon of defence. After the Arabs had watched our proceedings for a time with wonder, they came to the conclusion that we were engaged in sorcery, and requested us to leave the pyramid. As their remarks when interpreted to us were without effect, they wanted to use the power of the strongest to remove us from the top by violence. I withdrew to the highest point, and fully charged my strengthened flask, when

the Arab leader caught hold of my hand, and tried to drag me away from the position I had attained; at this critical moment I approached the top of my flask to within striking distance of the tip of his nose, which might be about 10^{mm}. The action of the discharge exceeded my utmost expectation. The son of the desert, whose nerves had never before received such a shock, fell on the ground as though struck by lightning, rushed away with a loud howl, and vanished with a great spring from our vicinity, followed by the whole of his comrades.

We had now a full opportunity of carrying out our experiments. Unfortunately we had made no preparations for them, and they were made more difficult by the ever increasing power of the wind, which made it difficult and even dangerous to a certain extent to stand upright. When I insulated myself from the stonework of the pyramid by means of an improvised insulated stool formed of bottles set up on end, the whizzing noise produced on raising the finger ceased after a short time. I could now communicate sparks to my comrades by approaching my hand to them, and I received a slight shock when I came in contact with the ground. On the other hand my hair stood less on end than that of my non-insulated comrades when I touched the ground. We unfortunately had not the means of determining exactly the kind of electricity. We tried to charge and discharge the bottle by means of a point made of tinfoil, so as to arrive at some conclusion by the phenomena observed as to the kind of atmospheric electricity, but obtained no satisfactory results.

It is worthy of remark that we observed the phenomena described only on the top of the pyramid. Only a few steps lower they were already very weak, and on the plain we could hardly detect any electrical manifestations. The wind blew there with equal violence, and there can be no doubt that they continued up above as before.

As the electrical phenomena were first observed when the desert sand reached the top of the pyramid, it must be considered as the carrier, and also probably as the cause of the electricity. If it be assumed that the particles of dust and grains of sand lashed by the wind were electrified by the dry surface of the ground of the desert, then each electrified grain must form one coating of a condenser of which the other was the earth itself, whilst the air between the

two formed the insulating material separating the coatings. Through the ascending motion of the particles of dust the insulating layer was strengthened, the striking distance of all these little charged flasks consequently increased, and at a height of 500 feet must be considerably greater than in the immediate neighbourhood of the ground. The electricity of the powerfully electrified dust cloud which lay above the surface was opposed by an equal quantity of electricity on the upper surface of the earth. The conducting pyramid must therefore exercise a very considerable condensing effect on the electricity on the earth's surface, because it must be considered as a gigantic point. It is therefore not surprising that the electrical difference between the highest and finest points on the summit of the pyramid, such as the raised finger or the neck of the bottle, and the grains of sand, was so great that numerous little sparks sprang out between them, whilst on the plain hardly any electricity was observable. In this way the observed phenomena may be fully explained.

PROPOSAL FOR A REPRODUCIBLE UNIT OF ELECTRICAL RESISTANCE.*

I WAS induced some years ago to undertake the experiments about to be described both on account of the want of a generally accepted unit of resistance, and of the actual inconveniences arising therefrom, especially as regards technical physics.

My original intention was to procure for Jacobi's unit of resistance a more general technical application. I soon found, however, that this could not be done without inconvenience. In the first place several of Jacobi's standards which I had procured differed so materially from one another, and agreed so slightly with the specified data of their resistance that it would have been necessary for me to have fallen back upon Jacobi's original standard, which was not, however, at my command. But apart from this, I was convinced that a unit of resistance was only suitable for general

* Poggendorff's *Anu. d. Phys. u. Chem.*, 1860, Vol. CX. p. 1.

use if it could be reproduced. It is not yet entirely decided whether the resistance of a metal wire alters in course of time, by being shaken in transport, by the passage of currents through it, and other influences. It is, however, very probable that such an alteration does take place, and hence it is not admissible to select the resistance of a special wire as the standard unit of resistance. Moreover by the continued copying of a unit of resistance from other copies (which would be unavoidable on its general introduction), the deviations from the standard would become continually greater. For researches copies are useless which are less exact than the improved instruments and exact methods employed. Lastly it is both desirable and convenient to be able to combine a definite geometrical notion with the unit of resistance, which can never be the case with a metallic wire, because the resistance of a solid body in large measure depends on its molecular constitution, as well as upon impurities in the metal which cannot easily be avoided. The absolute unit of resistance appeared to me to be equally unsuitable for general use. It can only be produced with very perfect instruments, in places specially fitted up for the purpose, and with great manual skill; and it is wanting in the physical representation so useful in practice. Finally the numbers it involves are very inconvenient on account of their being so large.

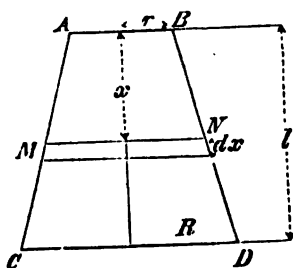
The only practical way to establish a unit of resistance sufficient for all requirements, and specially capable of being easily made by anyone with the necessary accuracy, appeared to me to be to employ the resistance of mercury as the unit. Mercury can very easily be obtained or rendered almost if not perfectly pure. So long as it is liquid it does not alter in its molecular condition so as to affect its conductivity; its resistance is less dependent than that of other simple metals on variations of temperature, and finally its specific resistance is very considerable, and hence the figures for comparison are small and convenient.

I therefore resolved to try whether by suitable means it was possible with ordinary commercial glass tubes, and purified mercury, to make definite units of resistance of sufficient accuracy. The greatest difficulty appeared to lie in the impossibility of procuring perfectly cylindrical glass tubing; ordinary commercial glass tubing being usually irregular in its internal diameter. By

calibration with a short thread of mercury it is however easy to select from a large number of glass tubes, pieces of a metre in length, the section of which varies pretty uniformly. The tube may then be treated as a truncated cone, the resistance of which can be calculated. The volume of the cone filled with mercury can be determined with great ease and accuracy by weighing the metal.

Let Fig. 23 represent such a truncated cone of length l , the

Fig. 23.



parallel bounding circles of which have the radii R and r . Assume a section MN of the cone with the radius z and thickness dx parallel to the plane AB and at the distance x from it. If W is the resistance of the cone in the direction of its axis, dW the resistance of the section MN in the same direction, then—

$$dW = \frac{dx}{z^2 \pi}$$

But

$$z = \frac{(R - r)x}{l} + r.$$

The value of z differentiated with regard to x is—

$$\frac{dz}{dx} = \frac{R - r}{l}$$

whence

$$dx = \frac{l}{R - r} \cdot dz.$$

By substituting this value of dx in the first equation we obtain—

$$dW = \frac{l}{(R - r)\pi} \cdot \frac{dz}{z^3}$$

By integration with respect to z we obtain—

$$W = \int_r^R \frac{l}{(R-r)\pi} \cdot \frac{dz}{z^3} = \frac{l}{(R-r)\pi} \cdot \left(\frac{1}{r} - \frac{1}{R} \right)$$

or
$$W = \frac{l}{R \cdot r \cdot \pi} \quad . \quad . \quad . \quad . \quad (1)$$

Next let V be the volume of the truncated cone, G the weight of the mercury contained in it, and σ its specific gravity, then—

$$V = (R^2 + Rr + r^2) \frac{l\pi}{3}.$$

Dividing by Rr we obtain—

$$\frac{V}{Rr} = \left(\frac{R}{r} + 1 + \frac{r}{R} \right) \frac{l\pi}{3}$$

and putting $\frac{R^2}{r^2} = a$

$$\frac{V}{Rr} = \left(\sqrt{a} + 1 + \frac{1}{\sqrt{a}} \right) \frac{l\pi}{3},$$

whence $Rr = \frac{V}{l\pi} \cdot \frac{3}{1 + \sqrt{a} + \frac{1}{\sqrt{a}}}$

Putting for V its value $\frac{G}{\sigma}$, we obtain—

$$Rr = \frac{G}{l\pi\sigma} \cdot \frac{3}{1 + \sqrt{a} + \frac{1}{\sqrt{a}}}$$

and substituting this value of Rr in equation 1 gives—

$$W = \frac{l\sigma}{G} \cdot \frac{1 + \sqrt{a} + \frac{1}{\sqrt{a}}}{3} \quad . \quad . \quad . \quad . \quad (2)$$

The value of W found in this way is evidently correct for a pyramidal form of conductor so long as a represents the ratio of the greatest to the least section. It is further correct when for a

single truncated cone of the length l , any number n of such cones of equal length are substituted, whose combined length is equal to l , if only in each the ratio of the greatest to the least section or its reciprocal is equal to a .

For in this case if—

$$l = n \lambda$$

where λ represents the length of one cone—

$$W = \frac{n \lambda^3 \sigma}{\frac{G}{n}} \cdot \frac{1 + \sqrt{a} + \frac{1}{\sqrt{a}}}{3}$$

or

$$W = \frac{n^3 \lambda^3 \sigma}{G} \cdot \frac{1 + \sqrt{a} + \frac{1}{\sqrt{a}}}{3}$$

or

$$W = \frac{l^3 \sigma}{G} \cdot \frac{1 + \sqrt{a} + \frac{1}{\sqrt{a}}}{3}.$$

As further the correction coefficient for the conical form of the conductor—

$$\frac{1 + \sqrt{a} + \frac{1}{\sqrt{a}}}{3} = \frac{1 + \frac{R}{r} + \frac{r}{R}}{3}$$

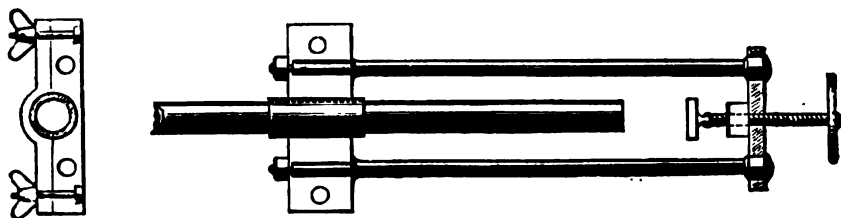
differs very slightly from 1, when R and r are nearly equal, each tube not perfectly cylindrical may be considered without any appreciable error as a truncated cone, and the ratio a determined from the quotient of the greatest and least length of the column of mercury used for calibration.

By means of a series of experiments I next ascertained whether the calculated values of the resistance of different tubes of very irregular mean sections agreed sufficiently with the measured values. The following was the method I employed :—

Ordinary commercial glass tubes of from $\frac{3}{4}$ to 2^{mm} internal diameter were fastened to a long scale ; a drop of mercury was

introduced into each of them, and the length of the thread of mercury so produced was measured. By inclining the tube, the small thread of mercury could be made to traverse its whole length step by step, and in this way the length of about a metre of each tube could be determined, which appeared most cylindrical or most uniformly conical. These pieces were then cut out of the tube, and the ends ground down by means of an apparatus constructed by Mr. Halske for the purpose till the tubes were exactly one metre long; the tubes thus prepared were then carefully cleaned, which was most easily effected by twisting together two thin silk-wound wires of German silver or steel, passing them through the tube, and then fixing a pad of cotton-wool to the projecting end of the wire, which was then carefully and slowly drawn through the tube. This operation always required some

Fig. 24.



care to avoid breaking the tube. The tube was then filled with purified mercury and the contents weighed. This was done in the following manner: One end of the glass tube was fixed by means of a vulcanized rubber cork into the mouth of a small retort receiver, such as are used in chemical laboratories, so that the end of the tube projected into the receiver. To the other end of the tube was fastened an iron clamp as shewn in Fig. 24, by means of which a smooth plate of iron could be screwed against the mouth of the tube. The receiver being suitably supported was filled with pure mercury, which was allowed to flow through the slightly inclined glass tube into a vessel placed below. When after a short time inspection showed that all the little air bubbles which at first existed had been removed by the flowing mercury, the lower opening was tightly closed by means of the screw moving the iron plate, the tube placed upright, and the other end

withdrawn from the india-rubber cork. If this were done with care the now vertical tube was found to be quite full, and the column of mercury terminating in a small projecting meniscus. The upper opening was now closed by pressing a plate of ground glass upon it, and the superfluous mercury removed. After any small drops of mercury clinging to the tube were removed with a brush the contents were emptied into a small glass vessel and weighed in an accurate chemical balance. When the precaution is taken of allowing the mercury to flow out very slowly by inclining the tube very slightly, and only removing the iron plate very gradually, no globules of mercury remain behind in the tube, which is usually the case without such precaution. Any heating of the tube when full by contact with the naked hand was of course avoided. The temperature was observed at the time the tube was being filled and the weight obtained reduced to zero temperature. Of the following tables, Table I. gives the different lengths of the thread of mercury used in calibrating the pieces of tubing employed, and the ratio α of the largest to the smallest

TABLE I.

1	2	3	4	5	6
125.0	101.2	48.2	143.0	115	111
116.4	98.4	47.5	145.0	116	109
115.3	96.9	45.0	146.0	119	107
114.0	94.5	45.0	145.0	121	105
112.0	94.0	44.8	143.5	121	105
110.2	93.3	44.2	142.5	122	103
108.2	94.5	43.9	142.5	121	101
107.0	95.7	43.7	140.0	120	100
107.0	97.5	42.5	139.0	119	101
106.0	99.4	41.0			102
	100.1	40.1			100
Therefore $\alpha =$ $\frac{125}{106}$	$\frac{101.2}{93.3}$	$\frac{48.2}{40.1}$	$\frac{146}{139}$	$\frac{122}{115}$	$\frac{111}{100}$
Hence the various coefficients of correction—					
1.00225	1.00055	1.00282	1.000261	1.000289	1.000906

section so obtained. Table II. gives the actual weight of the mercury in the tube and the reduction of the same to zero Centigrade.

TABLE II.

1	2	3	4	5	6
13·208	27·1915	24·3825	62·368	69·802	11·767
13·210	27·1900	24·3830	62·366	69·796	11·768
13·209	27·1915	24·3840	62·357	69·803	11·767
13·209	27·1920	24·3833			
at	at	at	at	at	at
13°·5 R.	14° R.	13°·5 R.	18° R.	14°·7 R.	15°·2 R.
			61·395	69·795	11·776
			62·398	69·795	11·777
			63·393	69·794	11·774
					11·774
			at	at	at
			14°·5 R.	18° R.	14°·7 R.
Weight in Grammes at 0°.					
13·2491	27·277	24·457	62·774	70·054	11·208

If in formula (2) for the resistance previously obtained—

$$W = \frac{\rho \sigma}{G} \cdot \frac{1 + \sqrt{a} + \frac{1}{\sqrt{a}}}{3}$$

the value of G in milligrams as taken from Tables I. and II. is substituted and the correction coefficient applied, and if the specific gravity of mercury at 0° is taken at—

$$= 13·557$$

and for the mean length of all the tubes—

$$l = 1000^{\text{mm}}$$

we obtain the resistance of the tubes expressed in units of the resistance of a cube of mercury of 1^{mm} side. Table III. gives these calculated values :—

TABLE III.

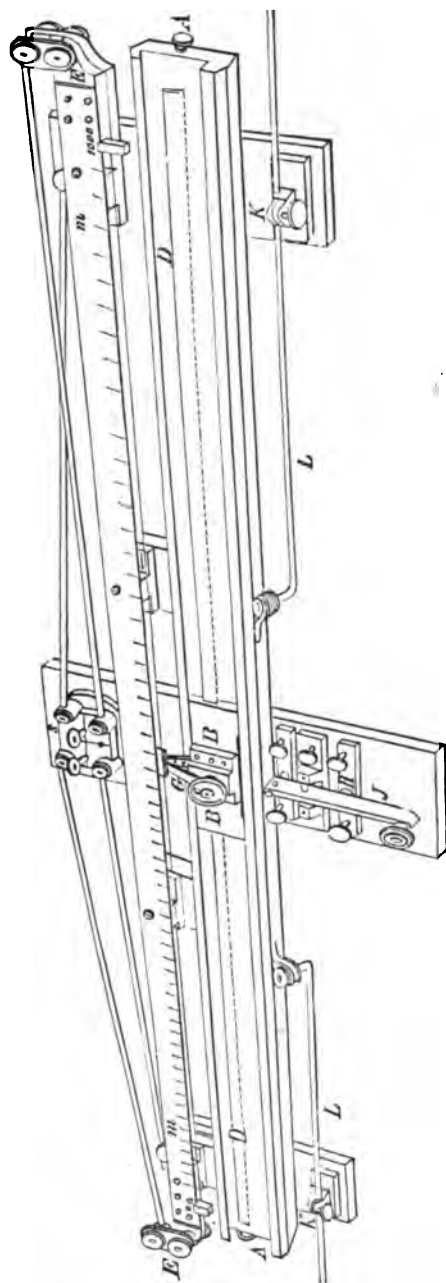
1	2	3	4	5	6
1025·54	497·28	555·87	216·01	193·56	1148·9

The resistance of these tubes filled with mercury at 0° was now compared with a copy of Jacobi's standard (B), which was effected by means of a Wheatstone bridge. As the Wheatstone bridge, which I used in the form given to it by Halske and myself, is adapted for very accurate measurements of resistance, its more detailed description will not be without interest.

Fig. 25 gives a perspective view of the bridge. A A is a brass bed, along which the slider B B is shifted. The rotatable head C of the slider is provided with a toothed wheel, which works in a rack S fixed to the bed. The slider may therefore be moved both directly and by turning the milled head. The insulated pieces E E and the scale *m m*, divided into millimetres, are also fixed to the bed. Between the insulated metal pieces E E, the inner surfaces of which are perpendicular to the scale and exactly $1,000^{\text{mm}}$ apart, is stretched a platinum wire of about 0.16^{mm} diameter. This wire, the ends of which exactly correspond with the divisions 0 and 1000, is gripped by two little platinum rollers, the axes of which are fixed to the slider by means of the spring G. The resistances to be compared are inserted between the metal plate H, which can be connected with one pole of the battery by means of the key I, and two thick copper rods L L, which slide in the terminals K K. The other pole of the battery, for which generally a single Daniell cell is used, is connected with the slider B and the platinum roller. The terminals K K and the metal pieces E E, which serve to secure the platinum wire, are joined up by means of thick copper rods with the four plates of the plug commutator S. By changing the two plugs the resistances to be compared can be interchanged. The ends of the coil of the galvanometer used are also connected with the metal pieces E E. In the following measurements a mirror galvanometer was used, with a circular steel mirror 32^{mm} in diameter, and wound with 36,000 convolutions of copper wire 0.15^{mm} thick. The distance of the millimetre scale from the mirror was $6\frac{1}{2}$ metres. The measurements collected in the following table were made for the most part by Dr. Esselbach with the apparatus described.

The following was the method employed. Each end of the glass tube to be tested was fixed by means of an india-rubber cork in the tubulure of the retort receiver. This receiver was so arranged that the unused wider neck was directed upwards, and,

Fig. 25.



together with the connecting tube, placed in a trough filled with pieces of ice. One receiver was then filled with purified dry mercury, which filled the tube and ran through it into the empty receiver. When the level of the mercury was the same in both vessels the tube was, as a rule, quite full of mercury and free from bubbles. Thick amalgamated copper wires were passed into the mercury through the necks of both of the receivers, and the resistance of the tube was then compared by means of the bridge just described with a Jacobi's standard resistance.*

The resistance of the connecting wires was determined by plunging both amalgamated copper cylinders into one vessel filled with mercury; but it was found to be negligibly small in comparison with the resistance of the tubes.

The experiments collected in the following table were so arranged that in the first position of the commutator the slider B B was moved until the galvanometer showed no permanent deflection when the key I was pressed down. The resistances to be compared were then interchanged by the commutator, and the slider was again adjusted. These two readings are given in the columns marked *a* and *b*. If the observations were free from error the sum of both should equal 1000, which in the majority of instances was very nearly the case.

It must, however, be here remarked that after a balance had been obtained, on completing the circuit a small deflection of some divisions was always observed, indicating a greater resistance of Jacobi's standard, which consisted of coils wound parallel. As on breaking the circuit an opposite deflection of the same amount occurred, it was evidently to be attributed to the extra current in the coils of the Jacobi standard. It was further observed that if the current was kept on the mercury became warm, although only one Daniell cell was employed. Owing to the slow oscillation and the great damping of the swings of the mirror used, the error thus occasioned could be easily eliminated by only

* At first we used iron tubes as conductors, instead of amalgamated copper wires. We found, however, that there was a very considerable surface resistance between the iron and the mercury, although the surface of the iron was quite clean. This resistance also occurred with unamalgamated copper, and was particularly great when the cylinder, after having been cleaned, had been exposed for some time to the air; so that the phenomenon is probably due to the film of gas condensed on the surface of the metal.

TABLE IV.

Tours.	1		2		3		4		5		6	
	a.	b.	a.	b.	a.	b.	a.	b.	a.	b.	a.	b.
Observed Resistances .	605.7	394.3	429.1	570.9	456	543.7	247.6	752.6	227.4	772.8	633.2	366.8
			429.0	571.1	456.3	543.6			227.3	772.8	633.15	366.85
					456.2	543.3					633.10	366.90
					456.2	543.6						
Mean value .	605.7	394.3	429.05	571.0	456.2	543.6	247.6	752.6	227.35	772.8	633.15	366.85
For $b = 1$.	1.536		0.7514		0.8392		0.320		0.2942		1.726	
W_1 . . .	1016.52		427.28		555.38		217.73		194.7		1142.3	
$\frac{W}{W_1}$. . .	1.008		1.00		1.0008		0.992		0.994		1.005	

allowing currents of short duration to traverse the instrument. The slider was always arranged so that on closing the circuit there was a slight deflection to the left, which in consequence of the heating due to a continuance of the current changed into a deflection to the right. By again slightly shifting the position of the slider the deflection to the left could be made exceedingly small, and the effect of the heating thus entirely removed.

The line denoted by W' is obtained by multiplying the preceding one by the number 661.8, which number is obtained by comparing the calculated resistance of tube No. 2 with the resistance of the Jacobi standard employed. The numbers in

Fig. 26.



this line ought consequently to agree with the calculated resistances of the tubes, as given in Table III. The line indicated by W contains the quotients of the calculated by the observed resistances, which show that the differences are not greater than were to be expected. The principal errors in our measurements have arisen from neither the temperature of the mercury nor that of the copper standard employed for comparison being quite constant. The temperature of the ice water varied between 0° and 2° ,

and that of the standard between 19° and 22° C. But as the conductivity of copper is diminished by heating by 0·4 per cent. per degree Centigrade, the differences, which do not amount to 1 per cent., are fully accounted for; and there can be no doubt that the method employed is available for the reproduction of standard resistances to any degree of accuracy.

The observed resistances of Table IV. ought properly to have been diminished by the amount of the resistance to the passage of the current in the mercury of the glass vessel, or of the surface resistance in passing from the orifice of the tube to the amalgamated leading wires. This resistance may without great error be considered as the resistance of a hemispherical cup, the inner radius of which is equal to the inner radius of the tube, and its outer radius is very great compared with r , and may therefore be taken in the calculation as infinitely great. The resistance of a hemispherical cup of the thickness dx and radius x may be represented by

$$dW = \frac{dx}{2x^2\pi}$$

whence

$$W = \int_r^\infty \frac{dx}{2x^2\pi} = \frac{1}{2\pi r} \cdot \frac{r}{2\pi r^2}.$$

The resistance to the passage of the current in both masses of mercury is therefore equal to the resistance of an increase in the length of the tube by half its diameter. If owing to the end surfaces of the tube being flat instead of hemispherical, as assumed in the calculation, a slight increase of the resistance is caused, yet the whole amount is so small that it can be conveniently neglected.

The straight tubes employed in the experiments previously mentioned are somewhat unsuitable for use as standards. I therefore got Mr. Geissler, of Berlin, to make some similar spiral glass tubes with turned up ends, and having little glass vessels provided for the reception of the leading wires. These spirals were fixed into the wooden cover of a wide vessel filled with water, as shown in Fig. 26. The temperature of the water was observed by means of a thermometer introduced through an opening in the wooden

cover. The filling of the glass spiral with mercury free from air bubbles was easily effected as follows. The mouth of the tube in one of the glass vessels was first closed by a suitable stopper, while the other vessel was filled with mercury; then the stopper was carefully raised, and only quite removed when the mercury had slowly passed through the whole of the windings of the tube.

As mercury is not found in the series of metals for which Arndsten* has determined the variation of the specific resistance with temperature, this deficiency had to be supplied in the first place, which Dr. Esselbach did by means of the arrangement described. The resistance of one of the spiral tubes was compared with that of the straight tube No. 2, first at the temperature of melting ice, and then at higher temperatures. If w represents the resistance of tube No. 2, equal to 498.7 in Table III., w_1 the resistance of the spiral tube; and remembering that the resistances of the leading wires to tube 2, and to the spiral were each made equal to 11 mercury cubes of 1^{mm} side, it follows that—

$$\frac{w+11}{w_1+11} = \frac{a}{b}$$

where a and b represent the lengths of the portions of platinum wire of the bridge, when no current passed through the galvanometer. This was the case when—

$$\frac{a}{b} = \frac{311.3}{688.7}$$

whence $w_1 = 219.4$.

The temperature of the straight tube was now maintained at 0° by means of melting ice, whilst the water surrounding the glass spiral was heated. In the following table t represents the temperature of the straight, t_1 that of the spiral tube, a and b the lengths of wire when balance was obtained, y the required coefficient, calculated by Arndsten's formula—

$$\frac{w_1(1+y t_1)+11}{w(1+y t)+11} = \frac{a}{b}.$$

* Pogg. Ann., Vol. CII. p. 1.

TABLE V.

t .	t_r .	a .	b .	γ .
0°	47° C.	320·4	679·5	0·000964
0	34·5	318·0	682·0	0·000960
0	16·5	314·6	685·4	0·000931
Mean . .				0·000968

It hence follows that of all the simple metals mercury is the one, the resistance of which is least increased by increase of temperature.

With the aid of this coefficient the resistances of both the other glass spirals A and B were determined, which were afterwards used as a standard for the preparation of copies of the resistance in German silver. The resistance of spiral A was 514·45 at 0°, and of spiral B 678·0.

German-silver wire is specially adapted for the preparation of standard resistances, because its conductivity is very low, and only varies with variation of temperature by about 0·04 per cent. per degree Centigrade according to Arndsten.

In the previous experiments the resistance of a mercury cube of 1^{mm} side was always employed as the unit of resistance. For small resistances, and especially for the calculation of resistance, this unit has many conveniences. It appears, however, to be advisable to bring the unit of resistance into more perfect accordance with the metric system; I therefore propose to employ as the unit of resistance, the resistance of a prism of mercury 1 metre in length and 1 square millimetre in section at 0° C.

If this proposal should receive general acceptance, then all specifications of resistance could be at once reduced to the metrical system. Each physicist would be able to get a unit of resistance as accurate as his instruments permit or require, and to check the variations of the resistance of the more convenient metal standards. In such case it must follow that mercury will in future be taken as the unit of conductivity, and not copper or silver as formerly. Unfortunately but few comparisons have been made of the conductivity of mercury and of the solid metals from which such a table could be calculated, and in most comparisons of the conductivity of solid metals it has not been stated whether

hard, drawn, or annealed wire was used. From the following table, however, it appears that the conductivity of annealed wires is considerably greater than that of unannealed.

1 Kind of Wire.	2 Length in Milli- metres.	3 Weight in Milli- grammes.	4 Specific Gravity.	5 Resistance at 0° tem- perature.	6 Con- ductivity. Mer- cury = 1.
1. Silver wire, hard .	4014·4	4884·9	10·479	614·55	56·252
annealed .	4014·4	4889·1	10·492	537·2	64·38
2. do. hard .	4014·4	3233·1	10·502	896·1	58·20
annealed .	4014·4	3009·6	10·5132	889·08	63·31
3. Copper, hard .	4014·4	3099·5	8·925	890·5	52·109
4. do. hard .	4014·4	4409·1	8·916	622·7	52·382
annealed .	4014·4	4355·2	8·903	599·05	52·013
5. do. hard .	2007·2	1260·4	8·916	545·8	52·217
annealed .	2007·2	1252·7	8·894	517	55·419
6. do. hard .	2007·2	1263·2	8·916	545·6	52·121
annealed .	2007·2	1241·5	8·894	520·8	55·338
7. Platinum, hard .	436·4	544·1	21·452	910·6	8·244
8. do. hard .	436·4	550·1	21·452	897·7	8·27
9. Brass, hard .	1003·6	1406·1	8·473	530·6	11·439
annealed .	1003·6	1397·3	8·464	451·7	13·502

It follows that the specific conductivity of annealed silver wire is 10 per cent., and that of annealed copper wire on the average 6 per cent. greater than that of unannealed silver and copper respectively. This increase is specially noticeable with brass. As the hardness of drawn wire depends on the amount of its stretching after the last annealing, the hardness and conductivity must vary even when the metal is perfectly uniform. The temperature at which the wire was annealed, the duration of the annealing process, and the rapidity of the cooling, all affect the value of the specific conductivity. Column 5 of the above table is calculated according to the previously explained formula—

$$W = \frac{\rho \sigma}{G} \cdot \frac{1 + \sqrt{a} + \frac{1}{\sqrt{a}}}{3}$$

The coefficient of correction $\frac{1 + \sqrt{a} + \frac{1}{\sqrt{a}}}{3}$, for conical shape, may be left out of account with metallic wires, as it does not differ essentially from 1; and so this method is evidently much

more exact than that formerly used, in which the mean diameter of the wire had to be ascertained by direct measurement, and the square of this value entered into the calculation, rendering the method still more inexact. In the method I employ, on the contrary, all data may be determined with great accuracy, especially the length, which here enters as the square.

If the above table is compared with that prepared by Arndsten, it will be seen that the observed mean value of the conductivity of unannealed platinum, viz., 8·257, and the least value of unannealed silver, 56·252, are exactly in the ratios given by Arndsten, whilst the resistance of copper in the Arndsten table corresponds pretty well with the annealed copper wire of mine. As the silver and platinum used by me and Arndsten were chemically pure, I have in the calculation of the following table taken the resistance of platinum and hard-drawn silver as a basis. The values taken from Arndsten's table are indicated by an (A), those observed by myself by (S).

TABLE VI.

Conductivity of metals at the temperature t compared with that of mercury at 0°C .

Mercury	$\frac{1}{1 + 0\cdot00095\ t}$	(S)
Lead	$\frac{5\cdot1554}{1 + 0\cdot00376\ t}$	(A)
Platinum	$\frac{8\cdot257}{1 + 0\cdot00376\ t}$	(A, S)
Iron	$\frac{8\cdot3401}{1 + 0\cdot00413\ t + 0\cdot00000527\ t^2}$	(A)
German silver	$\frac{10\cdot532}{1 + 0\cdot000387\ t - 0\cdot000000557\ t^2}$	(A)
do. annealed	4·137	(S)
Brass, hard	11·439	(S)
do. annealed	13·502	(S)
do. do.	$\frac{14\cdot249}{1 + 0\cdot00166\ t - 8\cdot00000203\ t^2}$	(A)

TABLE VI (*continued*).

Aluminium	$\frac{31.726}{1 + 0.008638 t}$	(A)
Copper	$\frac{55.513}{1 + 0.00868 t}$	(A)
do. hard	55.207	(S)
do. annealed	55.258	(S)
Silver, hard	$\frac{56.252}{1 + 0.008414 t}$	(A, S)
do. annealed	64.38	(S)

For the sake of convenience, I have given Arndsten's observed values with the correction coefficient given by him for increase of temperature. Whether these are the same for annealed and unannealed wires I have not been able to determine. The brass upon which I experimented contains, according to an analysis made in my laboratory, 29.8 per cent. of zinc and 70.2 per cent. of copper.

In conclusion, I would remark for the benefit of those who may wish to prepare standards in the manner described that it is necessary to warm the mercury for some hours under a layer of concentrated sulphuric acid mixed with a few drops of nitric acid, so that all metallic impurities as well as the absorbed oxygen which greatly increase the conductivity may be altogether removed.

ON UNITS OF RESISTANCE AND THE DEPENDENCE OF THE RESISTANCE OF METALS ON HEAT.*

IN opposition to the proposal made by me in this Journal of a reproducible unit of resistance, Mr. Matthiessen† has lately set up one of his own. Whilst I proposed to adopt as the unit of resistance, the resistance at 0° C. of a column of mercury 1

* Poggendorff's Ann. d. Phys. u. Chem., 1861, Vol. CXIII. p. 91.

† Poggendorff's Ann. d. Phys. u. Chem., Vol. CXII. p. 353.

metre long and 1 square millimetre in section, Mr. Matthiessen proposes to employ Weber's absolute unit as the general unit of resistance, to compare it with the resistance of a wire made of an alloy of gold and silver, and then to reproduce it by manufacturing similar wires from the same alloy.

The first portion of Mr. Matthiessen's proposal has at first sight much in its favour. On a closer examination, however, there are very important considerations against it. A unit can only fulfil its purpose when it can be made as accurate as the instruments by means of which it is to be compared with others. If an arbitrarily chosen standard capable of reproduction by means of copies is to be objected to, as is actually done by Mr. Matthiessen, then necessarily the directly manufactured fundamental unit must be capable of reproduction with such exactness that our most delicate instruments are unable to detect any difference. This however is, unfortunately, not the case with regard to the determination of absolute resistance according to Weber's method. It is also not to be supposed that the method can be so perfected as nearly to satisfy the above requirements, for the measurement of the current and of the electromotive force in absolute units must precede the determination of the absolute resistance, and all the errors introduced in these difficult determinations of units recur in the determination of the absolute resistance. Indeed it may be affirmed with certainty that even the most expert scientists, equipped with the most perfect instruments and localities, will not be in a position to make determinations of absolute resistance which would not differ from one another by several per cent. But so little accurate a unit would not even serve for technical requirements, and even if it were possible to determine the absolute unit of resistance with very great accuracy, we should still be without an absolute unit for the conductivity of bodies, and should therefore again have to choose an arbitrary unit of conductivity. It is, however, much more convenient and explicit to define the unit of resistance as the resistance of a prism-shaped body of the material which has been taken as the unit of conductivity. Besides these reasons the absolute unit of resistance is unsuitable for general use, because it is so impracticably small, and is not

based upon a simple geometrical notion. Great however as is the value of the absolute unit of resistance for many purposes and calculations, and important as it is to know the value in absolute units of all other units of resistance generally used, it cannot but be considered as unsuitable for a universal standard of resistance. Mr. Matthiessen confines himself to the statement, "that the absolute unit of resistance is and always must be the best," without giving reasons for this assertion, or giving figures which would render it possible to actually reproduce it by means of the gold and silver alloy. He only wishes to give a preliminary proof that wires drawn from the gold-silver alloy suggested by him were specially suitable for the exact reproduction of units of resistance and for the manufacture of standard resistance coils. But this proof has quite failed according to his own figures, for whilst his soft wire No. III. has a conductivity of 14.92 (that of hard silver wire being taken as 100) the soft wire No. VII. has a mean conductivity of 15.16 ; differences of more than $1\frac{1}{2}$ per cent. are thus shewn to exist.

But even if the least concordant results are set aside, differences still remain, which in most cases reach nearly to 1 per cent. As good instruments for measuring resistance permit of measurements being easily made which agree to within one ten thousandth, it follows from Mr. Matthiessen's own results that his proposal has entirely failed. Even if the conductivity of the alloy was always exactly the same, and the wires were perfectly cylindrical and homogeneous, small resistances could certainly not be manufactured with exactness by its means, for at the points of contact of the ends with the connecting wires variable resistances of considerable amount always arise.

The objections which Mr. Matthiessen brings against the use of mercury as a unit of conductivity and for the production of standard resistances are partly founded on the erroneous supposition that I suggested the employment of glass tubes filled with mercury as standards of resistance for continual use. But such is absolutely not the case. I have proposed to manufacture standard resistances of German-silver wire in the way which I have explained, having the resistance of the proposed mercury unit. German silver is anyhow much more suitable for the manufacture

of standard resistances than the costly alloy of gold and silver, because its conductivity is much less, and it varies less with alterations of temperature. The German-silver wire used by Mr. Halske and me for the manufacture of standard resistances and boxes has a conductivity of only 3·22, mercury being taken as 1, and its resistance increases on heating by only 0·000272 per degree Centigrade. Mr. Matthiessen's objection that the mercury must be constantly renewed, because it would be rendered impure by the copper wires dipping into it, cannot however be considered serious, for the slight trouble of filling the spirals with freshly purified mercury can easily be undertaken, when new standards have to be made or old ones adjusted. Moreover if we are satisfied with an exactness of 1 or 2 per cent., which Mr. Matthiessen considers sufficient, platinum or iron wires can be used instead of copper for the connections, for the surface resistance between mercury and solid unamalgamated metals is only considerable with measurements of greater amount. That my method really fulfils its object, however, viz., the direct production of standard resistances within any required degree of accuracy, may be proved by the measurements contained in the following table, which have been made with the greatest care, with the object of representing as exactly as possible the unit of resistance I have proposed, that is a column of mercury 1 metre long and 1 square millimetre in section at 0° C. The glass tubes were intentionally chosen of very various diameters, and filled with mercury from various sources, which was purified by heating with English sulphuric acid in the manner described.

TABLE I.

No. of Normal Tubes.	r_0 . Calculated Resistance of Normal Tube at 0°.	α . Reading of Bridge Wire.	Temperature.		r_t . Observed Resistance of the Spiral Wire.		
			t_1 .	t_2 .	S_I .	S_{II} .	S_{III} .
3	555·99	161·4	13·55	14·4	2886·48
7	1917·32	899·1	15·68	15·8	2886·45
3	555·99	884·45	14·85	16·15	...	889·48	...
7	1917·32	683	15·98	16·6	...	889·36	...
3	555·99	217·5	15·0	16·2	1998·0
7	1917·32	489·7	16·22	16·7	1997·22

The values in the last three columns headed w_0 are calculated by means of the formula—

$$w_0 = r_0 \frac{(1000 - \alpha) [1 + \alpha (t - t_1)]}{\alpha}.$$

The numbers in the column headed t represent the temperature of the straight standard tubes, t_1 that of the spiral tubes to be compared. Both were continually surrounded by water in motion. The value of α is taken as 0.001 instead of 0.00095 as previously given by me, as will be explained later. The agreement between the measured values of the resistance of the spiral tubes shows that the sum of the errors of observation reaches only $\frac{1}{2}$ per thousand in the case of the spiral tube S_{11} and accordingly the accuracy of the standard may be relied on within this limit. Both the standard and the spiral tubes were filled with mercury freshly purified before use. It is always advantageous to do this, although many comparative tests have satisfied me that the resistance of the mercury in the tube remains absolutely unaffected after a week's use either by the oxidizing action of the atmosphere or the effect of the dissolved copper in producing impurity.*

At this point I must refute the charge of a serious error which Mr. Matthiessen brings against me and rebut it on himself. Mr. Matthiessen, quoted verbatim, makes the following remark with regard to my work: "for traces of foreign metals (0.1 per cent. or 0.2 per cent.) cause a diminution in the conductivity of pure mercury, and not as Siemens says an increase." I really cannot understand how a mistake of this kind, so easily verified by experiment, could have been made. I must maintain my statement

* As Mr. Matthiessen lays stress on the difficulty of producing perfectly pure mercury in sufficient quantity, it would appear that he wishes to make out that in his opinion the very simple method I propose for the purification of commercial mercury is not adequate. In order to remove this doubt, Dr. Quincke was so good as to place at my disposal for a comparative experiment a quantity of mercury most carefully prepared by himself from oxide of mercury. Dr. Quincke satisfied himself by his own observation that there was not the least difference observable in the resistance of one of my spiral tubes when the purified commercial mercury in one of them was replaced with freshly prepared chemically pure mercury. Their conductivity could therefore not have differed by 0.0001, for my instruments measured such a difference accurately.

At the same time, Dr. Quincke convinced himself that the resistance of the spiral became less when the mercury was rendered impure by the admixture of copper amalgam; its conductivity, therefore, considerably increased.

as absolutely correct, at least as regards all metals experimented on by me, namely, silver, tin and zinc.

I consider myself justified in the general conclusion that the conductivity of liquid metallic mixtures is that of the component metals, in the liquid condition, and at the same temperature, separated from one another, and that the cause of the great diminution of conductivity of solid alloys is to be looked for in the solidifying process itself. The following experiments show that there is at least a great deal of probability in this assumption.

The resistance of a spiral tube filled with pure mercury was compared in the usual manner with that of a similar tube. The pure mercury was removed from the tube, which was filled with mercury in which zinc had been dissolved. After the resistance had been measured, the mercury was carefully collected, and the zinc in it was determined by analysis. The same experiment was frequently repeated with mercury containing less zinc. In the following table the first two columns, headed t and t_1 , give the temperatures of the spiral tubes surrounded by water, the one

t .	t_1 .	a .	w .	m .	λ .
18.3	18.8	492.6	1.0323	0	
20.1	20.5	357.5	0.7924	1.52	11.2
18.4	18.3	541.5	0.8464	0.76	12.7
20.3	20.6	529.8	0.8870	0.825	11.2

headed a the deflection, that with w the resistances of the spiral filled with impure mercury calculated from the previous data, the next column contains the per-centage of zinc found by analysis, and the last the calculated conductivity of the zinc.

The conductivity of the zinc was calculated according to the formula—

$$\lambda = \frac{100 \sigma (W - w)}{s \cdot m \cdot w} + 1,$$

in which W represents the resistance of the spiral filled with pure mercury, w that of the tube filled with mercury containing zinc, m the per-centage of zinc, s the specific gravity of the mercury, and σ that of the zinc.

The formula is derived from the consideration that the ratio of

than calculation makes it. It would therefore be still smaller in proportion to fluid zinc, whilst it is three times greater when both metals are in the solid condition.

Mr. Matthiessen * has proved in the case of potassium and sodium that the resistance of some metals increases suddenly on passing from the solid to the fluid state, but still the proportionately small differences which he obtained for these metals do not suffice to explain the large differences found here. In order to clear the matter up I filled a glass vessel placed in a stearine bath with pure tin. The tin melted at 224° C., according to the mercury thermometer used, which was not further tested, and completely filled the glass tube. After the tube had been heated up to 280° I measured its resistance, and then allowed it to cool slowly, the liquid stearine being kept in continual motion by blowing in air, and again measured the resistance after the temperature had remained constant for some time. These measurements are collected in the following table :—

No.	α .	t .	w_t .	$\frac{\Delta w_t}{\Delta t}$.	α .
1	639·6	280	389·22	...	
2	642·7	249	382·51	0·216	0·0026
3	647·05	226	373·21	0·404	0·0099
4	755·25	219·6	176·28	30·77	0·3772
5	767·5	183	157·48	0·514	0·0063
6	792·8	99·5	120·48	0·444	0·0054
7	802·65	66·5	106·8	0·413	0·0051
8	821·3	0	81·57	0·379	0·0046
9	817·15	13·9	87·07		

The numbers in the column headed w_t are calculated according to the formula—

$$w_t = w_0 \frac{1000 - \alpha}{\alpha} - l,$$

in which w_t represents the resistance of the tube at the temperature t , w_0 its resistance at 0°, α the reading of the vernier of the bridge, and l the resistance of the conducting wires to the spiral. The latter amounted to 111·95^{mm}, or small units.† The

* Pogg. Ann., Vol. C. p. 177.

† This measure of resistance is $\frac{1}{1000}$ of a unit, or the resistance of a cube 1^{mm} side.

numbers in column *a* are all means of two measurements made with the arms of the bridge reversed by a commutator without resistance. Measurements in which the sum of both readings differed by more than 0.5 in 1000 were rejected. The comparison resistance was maintained at 0° C. by means of ice. When filled with mercury the spiral had a resistance of 742.24. The conductivity of zinc is hence $\frac{742.24}{81.51} = 9.1$. The last measurement is a

preliminary control measurement, made on another day, according to which the conductivity of tin is 0.1. It follows from the numbers given in the column headed $\frac{\Delta w_t}{\Delta t}$, which contains the

mean increase of resistance per degree between consecutive temperatures, that the increase in resistance of solid tin increases in an ascending progression on approaching the melting point; that on passing the melting point a sudden increase in the resistance takes place, which reaches nearly $2\frac{1}{2}$ times that at 0° C.; and that as the fluid tin is further heated the resistance gradually diminishes again, and at about 45° above its melting point it is only half what it was at the freezing point. If the numbers in this column are divided by the resistance at 0° (81.57), the coefficient of increase of resistance for the respective intervals of temperature is obtained. A glance at the figures in the column marked *a* shows that they tend towards a constant on both sides of the melting point. It is probable that this value for solid tin agrees with that found by Arndsten for other simple solid metals. The value obtained for fluid tin should also be compared with the coefficient for mercury, but a proper basis for such a comparison is wanting, because mercury being fluid at 0°, its resistance at this temperature, with which the increase of resistance has to be compared by means of the coefficient *a*, of course already includes the increase of resistance caused by the passage into the fluid state. It may be assumed with certainty that such a sudden reduction of conductivity occurs with all the simple metals upon fusion, for this is not only the case with the three already experimented upon—potassium, sodium, and tin—but has also been observed by me with solid amalgams and easily fusible alloys. In the case of the last-named, however, the jump is much smaller than with tin, a peculiarity which, however, appears to belong principally to alloys, and which

is, perhaps the actual cause of their diminished conductivity. Clausius* has already shown that the resistance of all pure metals† is nearly proportional to the absolute temperature. In fact, the difference observed can be perfectly accounted for by small inequalities of the resistance at 0°, in consequence of slight impurities in and special softness of the metals compared. Mercury alone forms a decided exception. From the analogy of tin, solid mercury at a sufficient distance from the melting point may probably be included among the other simple metals, and Clausius's law would include all pure metals, with the limitation that they are far removed from their melting points. The variations in the neighbourhood of the melting point may be conceived as a gradual approaching to and termination of the melting process. The conductivity of all simple metals would hence be infinitely great at the absolute zero of temperature, or resistance would be a phenomenon accompanying temperature and quantitatively directly dependent on it. If this dependence of resistance on temperature or on the quantity of heat in bodies, which can now be affirmed of it without essentially deviating from facts, can also be established beyond the melting point, then electrical resistance might actually be considered as a heat phenomenon, and we should obtain a strong new connecting link between the two natural forces heat and electricity. Unfortunately there are at present too few experiments on the latent heat of fluid metals, their capacity for heat, and alteration with temperature, as well as on the resistance of fluid metals and metals heated to higher degrees of temperature to be able to prove this assumed direct inter-dependence.

In conclusion, I subjoin two tables of experiments, which prove that the increase of resistance of mercury, as well as of copper, may be considered as constant between the freezing and boiling points. The mercury was distilled, and shortly before use was heated, while being continually stirred, under a layer of English sulphuric acid containing a few drops of nitric acid. Two spirals filled with this mercury were placed in glass vessels containing water, surrounded with bad conductors of heat. The temperature of one vessel was kept as constant as possible, whilst that of the

* Pogg. Ann., Vol. CIV. p. 650.

† Iron always contains carbon, and cannot therefore be considered as a simple metal.

other was gradually raised by passing steam into it. The temperature was read by means of a Geissler's thermometer graduated to 1-10th of a degree. In order that the temperature of the whole mass of water might be uniform, air was continually blown into it, so as to keep it in rapid motion.

No.	t_1	t	a	w_1	Δt	Δw_1	$\frac{\Delta w}{\Delta t}$
1	16.93	18.51	308.4	890.73	18.51	14.55	0.78
2	17.34	0	304.9	876.18			
3	17.85	28.59	310.5	899.73	28.59	23.55	0.82
4	18.05	27.79	310.3	898.69	27.79	22.51	0.81
5	18.2	42.24	313.35	911.55	14.45	12.86	0.89
6	18.2	41.14	313.10	910.49	13.35	11.80	0.88
7	18.2	40.49	312.8	909.23	12.70	10.54	0.82
8	18.45	59.59	316.8	926.24	19.10	17.01	0.89
9	18.5	57.14	316.3	924.10	16.65	14.87	0.89
10	18.55	55.29	315.9	922.40	14.80	13.17	0.89
11	18.5	97.44	324.7	960.45	42.15	38.05	0.90
12	18.8	97.14	324.6	960.01	41.85	37.61	0.90
Mean . . .							0.86

The column headed t_1 gives the temperature of the comparison resistance, that headed t the temperature of the heated spiral reduced by the constant temperature (17.34) of the comparison resistance. The copper wire used in the measurements of the following table was about 0.2^{mm} thick, covered with silk, and loosely wound upon a small frame of ebonite. The ends of the wire were soldered to thick copper wires, which had a resistance of 11.9, whilst the leading wires of the mercury spiral, which was kept constant at 0° C. by means of iced water, had a resistance of only 1.8. The little frame wound with copper wire was put into a test tube filled with well-boiled oil, which was itself placed in a vessel filled with water. The thermometer, which reached into the frame, was therefore surrounded by the wire of which the temperature had to be determined. The heating of the water was effected by steam in the manner described. By regulating the supply of steam the temperature in the test tube was maintained quite constant for a long time. The resistance of the mercury spiral used as a comparison resistance was 1997.5 at 0°.

The numbers in the column headed $\frac{\Delta w}{\Delta t}$, which are obtained by

division of the numbers standing in the same line in the two preceding columns, and give the increase of resistance for one degree rise of temperature, agree sufficiently in both tables, and show that the curve of increase of resistance both with mercury and

No.	t_1 .	t .	α .	w_1 .	Δt .	Δw_1 .	$\frac{\Delta w_1}{\Delta t}$.
1	0	0.4	433.95	1520.8			
2	...	19.8	448.70	1615.94	19.4	95.14	4.90
3	...	38.1	462.15	1706.01	18.3	90.07	4.92
4	...	58.4	473.25	1783.96	15.3	77.95	5.09
5	...	67.4	482.90	1855.10	16.0	71.14	5.08
6	...	87.3	496.2	1954.24	19.9	99.14	4.98
7	...	19.8	448.70	1615.94			
8	...	49.9	471.00	1768.20	30.1	152.26	5.05
9	...	72.1	486.0	1878.49	22.2	110.29	4.97
10	...	91.4	498.2	1973.06	19.3	94.57	4.90
11	...	38.2	462.3	1708.43			
12	...	54.6	474.15	1790.83	16.4	82.40	5.02
13	...	70.3	484.7	1868.67	15.7	77.84	4.95
14	...	91.4	498.2	1973.06	21.1	104.39	4.94
Mean . . .							4.98

copper may be considered a straight line. If the mean increase of resistance is divided by the resistance at 0°, the co-efficient α is obtained, which is 0.000985 for mercury and 0.00329 for copper.

The coefficient originally given by me for mercury, 0.00095, as well as that communicated later by Mr. Schröder van der Kolk,* viz., 0.0008, were therefore both too small. The reason that the coefficient obtained for copper, 0.00329, is so much smaller than that given by Arndsten, 0.0036, may be that I made use of commercial copper, having a conductivity of 46.7, whilst perfectly pure copper obtained by melting electrolytic copper under hydrogen has the conductivity 56.4. I cannot understand why at the end of his paper Mr. Matthiessen makes use of the expression that it is far from true that the conductivity of both pure and commercial copper varies proportionately with the temperature, as usually supposed, for he does not support his statement by giving any experiments.

* Pogg. Ann., Vol. CX. p. 452.

STANDARDS OF RESISTANCE.*

THE desire to bring about unanimity in the matter of an exact unit of the resistance opposed to the passage of electric currents has determined the proprietors of the widely known telegraph manufactory at Berlin, to prepare a number of exact copies of the unit of resistance which Dr. Werner Siemens has proposed and described, viz., a column of mercury one metre long and one square millimetre in section at 0° C.†, and to send them to physicists and telegraph engineers with a request to use them in determinations of resistance.

Messrs. Siemens and Halske have also had the goodness to supply me with such a standard of resistance, accompanied by another in the form of glass spirals, suitable for the reception of mercury. Both instruments, as well as resistance boxes from 1 to 1000 units, arranged like a set of weights prepared in the above mentioned establishment, certainly effect their object in the highest degree and deserve the widest application.

ON THE HEATING OF THE GLASS WALL OF THE
LEYDEN JAR BY THE CHARGE.‡

As it appeared probable to me that the glass wall of the Leyden jar must be heated by its charge and discharge, I have arranged an apparatus, by which very slight heatings can be observed with certainty. The result of the experiments made with it fully corresponded with my anticipations. The apparatus is of the following construction. I had equally fine iron and German-silver wires covered with silk. These wires were then cut into lengths of about one decimetre, and each German-silver wire was soldered

* Notice by Herr Poggendorff in Poggendorff's *Ann. d. Phys. u. Chem.*, 1863, Vol. CXX. p. 512.

† *Pogg. Ann.*, Vol. CX. p. 1 and Vol. CXIII. p. 90; *Phil. Mag.* Mar., 1863.

‡ Monthly Report of the Berlin Academy, Oct., 1864. Poggendorff's *Ann. d. Phys. u. Chem.*, 1864, Vol. CXXV. p. 137.

to an iron wire. The wires so connected were laid on a plate of glass covered with a cement of colophonium and shellac, so that the soldered junctions of 180 wires took up a space of one square decimetre without coming into contact. The wires were melted into the cement by pressing them down with a hot iron, and so fixed to the plate. After the adjacent free ends of the wires were soldered to one another, thus producing a thermopile of 180 elements, a second glass plate also covered with cement was laid with its cemented surface on the first. By carefully heating, the cement between the plates was then softened, and a portion of it pressed out with the few air-bubbles it enclosed. The thermopile was consequently now enclosed in a cement surface free from air exactly in the centre of a glass plate 5^{mm} thick. The whole of the middle portion of the glass plate, covering the inner soldered junctions, was now covered on both sides with tin-foil about one square decimetre in size, which was connected with insulating conducting wires. Both of the free ends of the thermopile were connected with copper wires leading to a sensitive reflecting galvanometer. The whole apparatus, inclusive of the outer soldered junctions, was carefully protected from any variation of temperature. A short series of charges and discharges by means of an induction coil with about one-inch spark was quite sufficient to throw the spot quite off the scale of my galvanometer in the direction corresponding to heating of the junctions lying between the coatings. After the cessation of the series of charges this deflection returned very slowly to zero. It disappeared altogether only after many hours. It is independent of the direction of the charging current and apparently proportional to the number of charges and to the sparking distance to which the condenser was charged. The deflection begins immediately after the first charge and then continues regularly. If, however, one of the coatings is touched with the finger, the scale remains motionless for two or three seconds before it begins its motion, which generally does not stop until it has passed outside the field of view.

The heating observed cannot arise through conduction of the mass of the glass, nor through its compression by the attraction of the coatings, nor lastly, through the penetration of electricity into the mass of glass lying next to the coatings. The first suggestion is directly negatived by the arrangement of the apparatus

and the experiments described. The heating through compression would be balanced by equally strong cooling produced by expansion, and could therefore produce no lasting heating even if the very slight attraction sufficed for it. The cause of the heating can be sought just as little in the penetration of the electricity into the mass of the glass lying next to the coatings, for the deflection could then not begin directly but only after an interval of a few seconds. If on the other hand we assume with Faraday that the charge and discharge depend on the occurrence of molecular motion in the insulator separating the coatings, there remains nothing remarkable in the fact of the heating of the insulator.

ON THE UNIT OF ELECTRICAL RESISTANCE.*

IN 1860 I published in this Journal a method by means of which I succeeded in constructing exact standards of resistance, and proposed to assume as the unit of electrical resistance, the resistance of a column of mercury one metre in length and one square millimetre in section, or the millionth part of the resistance of a cube of mercury of one metre side at the temperature of 0° C., and also the specific resistance of mercury as the unit of specific resistance of bodies. The grounds upon which I supported my proposal were briefly the following: That it is not advisable to adopt as the unit of resistance a material unit arbitrarily chosen, or one more or less inseparable from some natural unit to be deposited somewhere like the normal metre, and to be reproduced from copies, for there is no sufficient guarantee that its resistance may not vary; but even if one could be sure of the permanency of such a standard unit, the frequent copying and recopying of copies which would be unavoidable, coupled with alterations in its resistance which are very likely to occur, would cause faulty standards to be distributed, as was the case to so great an extent with the copies of Jacobi's normal standards.

The assumed unit of resistance must therefore be definable, or

* Poggendorff's *Annalen d. Phys. u. Chemie*, 1866, Vol. CXXVII. p. 327.

an absolute unit which can be reconstructed at any time and any where. Weber's dynamical unit of resistance would be available as such for scientific purposes, if it could be reproduced with the necessary exactness, viz., that required in the comparison of different resistances. But as this will probably never be the case, Weber's unit cannot actually serve as a general unit of resistance, although it is of course of the greatest importance to define as exactly as possible the relation between the unit chosen and Weber's unit. In arranging a general unit of resistance, its practical advantages and not the scientific harmony of the whole system of measurement must be considered as most important; the determination of resistances, combined with dynamical values, occurs but very seldom, and then only in strictly scientific instances, whilst in by far the larger number of cases, the resistance of bodies of different sizes, forms and materials are compared, and hence a measure of resistance founded upon a physical is to be preferred to one founded upon a dynamical basis.

It is on these considerations that the unit of resistance previously proposed by me is to be recommended, in which the metre is used as the unit of length, and mercury as the conductor which certainly best serves as the measure of conductivity, and which can be reproduced with sufficient accuracy with ordinary care, and when special care is taken with almost unlimited exactness.

I have not yet found any disproof of these principles which really enters into the question; on the other hand, Dr. Matthiessen proposed in 1861 to use as a reproducible unit of resistance, a certain alloy of gold and silver instead of mercury, and in the same year the British Association appointed a Committee to report to the Society on the most appropriate unit of resistance.

No one will have much predilection for the proposal of Mr. Matthiessen, who is practically acquainted with the great technical difficulties attending the production of a homogeneous alloy of uniform quality throughout, the preparation from it of wires having the same gauge and hardness, and its division into lengths without stretching and bending the wire, and finally soldering the ends to the thick leading wires without affecting the resistance; but as later on Mr. Matthiessen relinquished it in favour of a method proposed by the British Association of which he is a member, I need not make further reference to it.

This Committee has presented three reports for 1862, 1863, and 1864 to the Association, in which the theory of Weber's system of measurement is very clearly explained, so as to include the idea of the unit of work proposed by W. Thomson. The great importance to science of a general introduction of this systematic and coherent system of measurement is convincingly explained, Thomson's method for the determination of the $\frac{\text{metre}}{\text{second}}$ unit already mentioned is developed, and the arrangement and result of the experiments made, explained in detail. The names of W. Thomson and Clerk Maxwell are sufficient guarantee of the high scientific merit of this work. In fact, it has succeeded in determining resistance in Weber's absolute units far more exactly than W. Thomson and W. Weber previously did. But the Committee has nevertheless come to the conclusion that Weber's measure of resistance itself is not a suitable unit of resistance. Already in the first report it was proposed to employ as the unit a material standard of resistance, which should be as near to 10^{10} Weber's units or $10^7 \frac{\text{metre}}{\text{second}}$ units as is possible with the means at present at our disposal. This material unit is to remain unchanged and under the name of the British Association unit or Ohm is to become the future general unit of resistance. From time to time new determinations of this unit in Weber's absolute unit are to be made and reduction coefficients published for use in dynamical calculations. Professor Clerk Maxwell, Dr. Matthiessen and Fleeming Jenkin, who form the sub-committee entrusted with the preparation of the normal standards and the copies to be prepared from them, consider they have met the objection that the resistance of the normal standard might change, by the construction of ten different normal standards from alloys of the precious metals and from mercury, and copies from an alloy of platinum and silver. According to Dr. Matthiessen's experiments the resistance of these alloys will not alter, whilst with other metals and metallic alloys material changes have been found by him to take place at the end of two years.

I do not desire in any way to undervalue Dr. Matthiessen's experimental results, but I do not think that his statement that alloys of silver and gold or platinum are unalterable is to be considered

as so well founded and so absolutely reliable that a permanent normal standard of resistance could be based upon it. It seems strange that Dr. Matthiessen should have observed such marked variations in the conductivity of German-silver in short intervals of time, whilst I have found this alloy to be remarkably constant. This proves that as regards conductivity several unknown factors enter which will only be discovered after lengthy research. The circumstance that a gold chain has never been observed to become brittle can hardly have been seriously brought forward by Dr. Matthiessen as an argument in favour of the permanency of alloys of gold and silver. But it may be readily conceded that changes in the resistance of the normal standard and copies of it will be so small as not practically to affect our present experiments. The standard unit of the B. A. is intended however for future times as well, in which most likely infinitely higher claims to exactness will be required in a unit than we require. On this ground it is indeed very significant that the Committee prepared ten normal standards instead of one, although as asserted they correspond at present within about 0.03 per cent. But even if the B. A. unit agrees in value with the $10^7 \frac{\text{metre}}{\text{second}}$ unit to within 0.1 per cent., as stated in the report of 1864, the agreement is yet too small for the British Association unit to be considered in future as equivalent to the $10^7 \frac{\text{metre}}{\text{second}}$ unit, and if a coefficient of reduction has to be employed it seems unimportant whether it differs more or less from unity. Besides, it is not yet proved that there actually is this important agreement between the British Association unit and the $10^7 \frac{\text{metre}}{\text{second}}$ unit. A glance at the table of results given in the report of 1864, shows that between the two sets of figures combined in pairs differences exist which amount to over eight per cent., and even the mean values of these pairs differ by 1.4 per cent. I do not understand how the sub-committee considers itself justified in arriving at a probable error of only 0.1 per cent. with so great a difference between the separate measurements. Whatever method is employed to calculate the mean value of the given numbers much greater differences are arrived at if some very irregular measurements or some mean values are left out. In my opinion

safety only lies within the values not considered faulty and so retained.

It cannot, however, be inferred from the series of trials in question that so exact an agreement exists between the values of the B. A. unit and the true $10^7 \frac{\text{metre}}{\text{second}}$ unit, as is assumed by the sub-committee; and when it is remembered that the figures in the table have been obtained by the same observers using the same apparatus, and the same constants and correction coefficients, the probability is that the difference is actually much greater than it appears.

The determinations of the constants are declared to be exact within 0.0001; but it must be assumed that this was only attained as the result of careful manipulation and well-chosen methods of measurement. It is well-known that it is quite impossible to wind a silk-covered wire into an approximately round and solid coil without perceptibly stretching it. The amount of this stretching varies between 1 and 6 per cent., according to the thickness of the wire and the amount of tension employed in winding. It is therefore impossible to determine accurately within about $\frac{1}{2}$ per cent. the length of the coiled wire. The actual length, however, is given as 311.2856 metres. It is besides impossible to wind such a coil of covered wire circular and concentric; and therefore to determine exactly the circumference, the mean radius, and the thickness of the coil. Yet these values are given to the thousandth of a millimetre, and are considered trustworthy to the ten-thousandth part of their value. Whether the magnetic moment of the suspended needle, and the contemporaneous horizontal component of the earth's magnetism can be determined with the same degree of accuracy must remain undecided, but I do not think it possible.

As already said, I have no intention of asserting that the measurements were not actually made with the accuracy that is stated; but this can only have been the result of processes which are not of universal application.

Until these experiments have been repeated in other places with new instruments by different observers, and these results being compared with those of the sub-committee it is proved that a closer agreement exists, I consider that I am right in maintaining

that the agreement between the B.A. unit and the $10^7 \frac{\text{meter}}{\text{second}}$ unit is not closer than some few per cent.

On these grounds I still maintain the objections I have recapitulated against the adoption of the material standard of the sub-committee as the basis of a general unit of resistance ; but, in doing so, I do not in the least ignore the great importance of the determination of the Weber unit of resistance, which has been made as exactly as possible by the British Association. I am, on the contrary, of opinion that science has materially profited by this valuable work.

I believe, however, that the committee would have done better after having decided that Weber's absolute unit was not actually suitable as a unit measure of resistance, not to have set up another arbitrary unit, but to have had the $\frac{\text{metre}}{\text{sq. millimetre}}$ mercury unit, or shortly the metre-mercury unit, as defined by me, reproduced with all possible exactness ; to have distributed copies of this unit, which is already widely applied as specially suitable for practical requirements, and to have had the coefficient necessary for its reduction into Weber's absolute dynamical unit determined as exactly as possible. The committee would thus have been in accord with Kirchhoff's proposal, which it declared in the first report that it wished to follow, Kirchhoff having expressed himself in his letter published in the appendix to the first report as favourable to the maintenance of both units, and not to the exclusive use of Weber's system, as more recently stated, to which William Weber himself told the author he was opposed. My own early experiments, and especially the more recent and careful measurements of Robert Sabine, prove that an exact determination of the mercury unit by the British Association, with the rich means and considerable forces at the disposal of its committee, would have rendered it serviceable for all present requirements, that is, for the comparison of two different resistances. A more exact determination of the mercury unit would become necessary in the future as the exactness of physical measurements advanced ; but this would hardly give rise to any inconvenience, as the true value of the unit is undoubtedly fixed, the differences in their reproductions being so small as to be

entirely negligible in ordinary measurements of resistance ; while for exact measurements, the standards employed would have to be tested on account of probable variation.

Unfortunately the committee entrusted with the production of the B. A. unit and the reproduction of copies has not followed the way I suggested, whilst members of the committee, Messrs. Matthiessen and Fleeming Jenkin, have attacked my proposals both in the Reports of the British Association and in special memoirs published in more easily available journals, in a way which I think has not hitherto been customary in scientific criticism. The plan followed by them in common consists not in examining my proposal on its merits, but in representing my work as untrustworthy and uncertain.

Dr. Matthiessen sets up the two propositions :—

1. That no correct mercury unit has yet been issued.
2. That the units issued from time to time do not represent the same value.

He seeks to justify both propositions by asserting that I have not employed the correct specific gravity of mercury in my calculations ; that two resistance scales exhibited in the London Exhibition of 1862 differed by 1·2 per cent. ; that in my first determination of the mercury unit differences of 1·6 occurred, and that his experiments do not agree with mine.

Regarding the first assertion, Mr. Matthiessen overlooks the fact that the unit of resistance proposed by me depends on a definition, and is, therefore, absolute. I have never asserted that the standards of resistance which I prepared entirely correspond with this exact unit ; on the contrary, I have frequently expressed the wish that physicists already experienced in exact measurements would manufacture standards according to the very easy and safe method specified by me, which should agree with my definition as exactly as our present means will permit of. Mr. Matthiessen would only have been justified in his assertion had my definition not been distinct or the proposed method uncertain or faulty ; but he has neither made nor proved any of these objections. But whilst stating that his propositions are erroneous, I am ready to admit, on the other hand, that the specific gravity of mercury which I used in my calculation is not correct. When in 1858 I made the first experiments to ascertain whether the mercury unit

could be reproduced with sufficient accuracy, I got hold of the number 13·557 and used it as correct, as it had been proved by direct comparison of the heights of columns of water and mercury in tubes in communication. Unfortunately in the unit more recently reproduced and made with greater care and improved instruments this coefficient was retained, and not the number given by Regnault, 13·596, the correctness of which has since been frequently proved. Hence the standard formerly issued is 0·287 per cent. too great,* and if we take the coefficient of increase of the specific resistance = 0·00272 † of the German-silver wire used for the resistance standard, these will not represent the mercury unit at the temperature stated, but at a temperature 10·5° C. lower. Mr. Matthiessen has rendered an undoubted service in having shown the necessity of this correction, which however, as before mentioned, has nothing to do with a determination of the merits of the mercury unit.

Mr. Matthiessen states further that the mercury units made from time to time do not represent equal resistances. Mr. Matthiessen is aware that the mercury units have been determined in my laboratory at three distinct periods of time, and on each occasion with a nearer approach to the exact value. Mr. Sabine has given the variations of these three determinations in the following table :—

No. of Tubes.	Original Determination. 1859.	First Reproduction. 1880.	Second Reproduction. 1883.
3	555·37	555·99	556·05
5	193·56	193·73	193·73
7	...	1917·32	1917·54
8	...	2600·57	2601·46

The greatest difference between the first and third determina-

* According to the formula employed—

$$W = \frac{l^2 \sigma}{Q} \cdot \frac{a + \sqrt{a} + \frac{1}{\sqrt{a}}}{3}$$

in which W represents the resistance of the normal tube, l its length, Q the weight, σ the specific gravity of mercury, and a the ratio of the greatest to the least section of the tube.

† Pogg. Ann., Vol. CXIII. p. 4.

tions reaches only to 0·1 per cent., and not nearly to ·2 per cent. as stated. From the first determination only a few standards and resistance boxes were prepared for our own use. The resistance boxes prepared and employed for technical purposes were manufactured according to the second determination. From the mean value of the third determination I have had about a hundred standards made each of one unit, which I have presented to eminent physicists, technologists, and scientific institutes, in the hope of bringing about the general adoption of a rational measure of resistance.

These standards were exactly alike when sent out, and unless they have since altered agree to within 0·05 per cent. with the true mercury unit, when, as already stated, they are measured at a temperature 10·5° C. lower than specified. Standards of resistance other than those marked by Messrs. Matthiessen and Jenkin with "Siemens, 1864," were not distributed by me.

Mr. Matthiessen supports his assertion that the units prepared by me do not represent the same resistance from measurements made by Mr. Jenkin (who acted as a juror of the London Exhibition of 1862) with two sets of resistance coils arranged like a set of weights and ranging from 1 to 10,000 units. I do not know whether Mr. Jenkin measured correctly, as he found a difference between the coils of 1·2 per cent.; but it is quite incomprehensible to me how Dr. Matthiessen could think of comparing sets of resistances for technical purposes with standard measures, or how he could found so grave a charge as he has made exclusively on the uncorroborated evidence of an exhibition juror. He ought to know that the places of contact of solid metals always produce an alteration in resistance, and that therefore the twenty contact plugs through which the current must pass wholly or in part must have a considerable influence on the exactness of the measured resistance. He should besides be able to appreciate the great difficulties connected with the correct summing up of 10,000 units. The scale described by Mr. Matthiessen as "Siemens, London," was one of the first prepared for our own technical purposes in 1859, according to an imperfect summing method and arranged like a set of weights. It formed one of the branches of a so-called measuring bridge, with which the resistances were measured during and after the laying of the Indian Cable through

the Red Sea, and was admitted into the Exhibition on account of its historical interest, as with its help exact measures of resistance of submarine cables were for the first time substituted for the valueless statements about currents previously given. These older bridges have been subsequently re-adjusted, so as to agree with those made in accordance with an improved method, and marked "Siemens, Berlin," by Messrs. Matthiessen and Jenkin. Mr. Matthiessen also asserts that the more lately prepared sets of resistances were about 0.5 per cent. greater than the resistance standards prepared by me in 1864. He comes to this conclusion from the resistance of a copper wire, which Mr. Jenkin compared during the Exhibition of 1862 with the sets of coils. The temperature is not given at which the copper wire was measured at these four-year intervals; if the temperature varied by only $1\frac{1}{2}^{\circ}$ C., the whole difference is explained.

In any case Messrs. Matthiessen and Jenkin were not entitled to utilize a single determination made by themselves of so doubtful and uncertain a character, to enter in all the tables of the report of the committee as well as in their own papers the two other columns marked respectively "Siemens, Berlin" and "Siemens, London," next to the column marked "Siemens, 1863," and in this way improperly to make it appear that standards of the mercury unit of such different resistances had been sent out by me.

Analogous to this is the reiterated statement that differences of 1.6 per cent. occur between my determinations of the mercury unit, and that this must therefore be the possible limit of exactness. Certainly in my first published memoir on this subject in 1860 there was such a difference, and I explained at the time the reason of it, viz., variations in temperature of the copper wire used for comparison of 3° C., and of the normal tubes filled with mercury of 2° C. Besides, as the object of the experiments described was not to produce standard units but to prove that the proposed method was suitable for their manufacture, tubes which were only slightly cylindrical were designedly chosen. For practical purposes an exactness of $\frac{1}{2}$ per cent. was then sufficient; Mr. Matthiessen was himself contented at that time with determinations of the specific conductivity of metals which differed many per cent. from one another.

The standard resistances which I have distributed are all

adjusted from the value of the third reproduction which was made by Mr. Sabine. A glance at his memoir* will show that this determination was made with the greatest care, and that the agreement between the prepared standards and the true mercury unit, which I state to be within 0.05 per cent., does not depend on doubtful mean values, but that all the standard tubes give similar results within this limit. Mr. Matthiessen now opposes to these measurements some of his own which have given a value 0.8 per cent. higher. He has nowhere given any reason for this difference, or for my method or Sabine's measurements being uncertain. But then he should at least have done his work with equal care, and the method we employed, if he did not wish to follow it exactly, ought not to have been spoilt in essential points. Mr. Matthiessen proposes a formula for correcting the conical form of the tubes which is not so correct as mine, as he assumes the tube to be composed of cylindrical instead of conical pieces, thus making the calculated mean section too small, and the calculated resistance of the tube consequently too great. Besides this he fills the tube by dipping it into a vessel of mercury, and lifts it out with the ends closed between his fingers, so that the ends of the tube are filled with the soft skin of the hand instead of with mercury, and the contents of the tube are too small, and the calculated resistances consequently too great. An error in the same direction would also occur if Mr. Matthiessen has omitted to take the precaution of changing the resistances to be compared and of only considering those measurements as reliable which are together equal to 1000, for otherwise erroneous measurements very easily take place through the heating of the thin platinum wire of the bridge.

If these errors committed by Mr. Matthiessen in his reproduction of the unit do not fully explain the important difference of 0.8 per cent. they bear sufficient testimony to the small amount of care he took in making the determination, and his results therefore cannot be brought forward in proof of the incorrectness of mine, or of the more comprehensive and exact determinations since made by Mr. Sabine.

Mr. Jenkin does not bring forward any new points of view in

* Printed in Pogg. Ann., Vol. CXXVII.

his paper "On the new unit of electrical resistance adopted by the British Association," but extends the application of Mr. Matthiessen's conclusions and experiments. His communication that four of the standards made by me in 1864 were compared by four different observers with four copies of the British Association unit, with the resulting values 1·0456, 1·0455, 1·0456, and 1·0457 is interesting. The mean value of these observations multiplied by the correction coefficient for the correct specific gravity of mercury, *i.e.*, $\frac{13\cdot596}{13\cdot557} \times 1\cdot0456$ or 1·0486 is therefore

the value of the British Association unit in mercury units, or 1 mercury unit = 0·9536 British Association unit.

In the uncertainty still proved to exist of the proportion between the British Association and the $10^7 \frac{\text{metre}}{\text{second}}$ unit, we can convert a resistance given in mercury units with the greatest accuracy now attainable into 10^{10} Weber's units or $10^7 \frac{\text{metre}}{\text{second}}$ units by deducting 5 per cent.

I must make some remarks on the historical sketch of the order in which proposals of units of resistance were made, and on the progress made in the work of measuring resistance, with which Mr. Jenkin commences his paper, to correct some errors and omissions concerning myself.

Complete sets of resistances from 1 to 100 units have been manufactured ever since 1848 in the Berlin establishment of Messrs. Siemens and Halske, and have been frequently described and distributed abroad; each unit was equal to the resistance of a geographical mile of copper wire 1 line in diameter at the temperature of 20° C. Mr. Jenkin says that: "Until 1850 measures of resistance were confined with a few exceptions to the laboratory; but as about this time underground and shortly afterwards submarine telegraph cables were introduced, the practical engineer soon learnt of what importance to him was a knowledge of electrical laws in their examination and arrangement."

Mr. Jenkin must surely know that underground lines of considerable length were laid down in Germany in 1847 and 1848. In the construction of these lines, and in the determination, by the methods described by me, of the faults which unfortunately

only too frequently occurred, the practical engineer had therefore at that time many opportunities of making exact measures of resistance, and of appreciating the utility of a knowledge of natural laws. Complete sets of resistances from 1 to 10,000 units arranged like a set of weights were employed by my brother, C. W. Siemens, and myself in the cable tests on which we were engaged in England in 1859. It will be in the recollection of Mr. Jenkin that he himself made the tests of the India cable at Birkenhead under my direction with such sets. He should not have forgotten in his historical sketch to have stated that already in our report of the Red Sea cable in 1859 the states of the conductor and of the insulator were given in mercury units, and that the method we then employed to measure the resistance which the insulator offered to the electric current, and to compare it with the resistance calculated from the specific resistance of the insulating material, forms the basis of that rational system of cable testing we introduced, which, with slight variations as regards methods and instruments, is still in general use.

Mr. Jenkin should further not have altogether passed over in silence the paper* my brother read at the 18th meeting of the British Association, in which our system of testing cables, before, during and after laying, and the determination of faults by measuring the resistance, is exhaustively treated. I am not aware of any other methods for the determination of faults besides those proposed by me.

Mr. Jenkin (without referring to any publication) gives Marié-Davy the honour of having proposed mercury "as a material suitable for a standard measure," and only gives me the merit "that my coils and apparatus prepared with the greatest care had materially improved the exactness of observations." But he omits to state that those who had previously drawn attention to mercury as a suitable material had given no method by which exact standards could be produced from it.

Mr. Jenkin must himself admit that his historical sketch is remarkably incomplete.

* Outline of the principles and practice involved in dealing with the electrical condition of submarine electric telegraphs, by Werner and C. W. Siemens. Report of the British Association, Oxford, 1860.

ON THE LAW OF THE MOTION OF GASES IN TUBES ;
ON THE PNEUMATIC DESPATCH OF MESSAGES
IN BERLIN.*

THE question whether the transmission of letters, despatches, &c., through tubes by means of pneumatic pressure can be usefully applied on a large scale, and what is the most advantageous construction of the tubes, of the stations, arrangements of the carrier for the packages to be forwarded, and finally of the pneumatic apparatus, depends essentially on the law of motion of air in tubes. Without knowing this exactly, without having determined the extent of the influence of the different factors limiting the velocity of the motion of the air in all portions of the conducting tube, there is no definite basis of construction, and one does but grope in the dark. There are certainly a number of formulæ for the motion of gases in tubes ; but they are all based on experience gained with very low pressures, and relatively very wide tubes, and appear inapplicable to narrow tubes and great differences of pressure such as are necessary when considerable velocities are aimed at. It was therefore necessary in the first place to determine the law of motion of gases in tubes by means of experiment.

The experiments, of course, could not be carried out in the short time available with perfect scientific strictness. One was limited to tubes of small diameter and short length, and the differences of pressure could not exceed the maximum of one-third of an atmosphere. As, however, a practical aim had to be considered, the approximate formulæ thus attainable were sufficient. The following was the method employed :

Drawn lead tubes of various diameters and various lengths were employed. By means of a pump with a flywheel and crank, which could either be used as an exhaust or compression pump, or simultaneously as both, the air was rarefied or compressed in a reservoir large in proportion to the volume of the pump cylinder. The reservoir communicated with the atmosphere through the tube in which the velocity of the air was to be

* Journal of Germao-Austrian Telegraph Society, 1866, Vol. XIII.

measured. The pressure in the reservoir was measured by means of a mercury manometer. It was then easy to turn the crank of the pump at such a speed that the pressure in the reservoir could be maintained constant, so that in the same time the same quantity of air could always be pumped into the reservoir as was discharged by the tube, or *vice versa*. The tube ended in a carefully constructed gas-meter, which measured the exact quantity of air that passed through the tube in a given time. The measured quantity of air, divided by the area of the tube, gave the velocity with which air of atmospheric density passed from the tube into the gas-meter when there was a higher pressure in the reservoir, or, on the contrary, the velocity with which it entered the tube when the pump acted as an exhaust pump. As the same quantity of air must always pass in and out in the same time, when the flow has become uniform its velocity at the other end of the tube can be easily calculated from the measured quantity of air by means of Mariotte's law. If, for instance, the air in the reservoir was rarefied to half an atmosphere, and if the velocity of air at atmospheric pressure was 50 feet on its entrance into the tube, the same quantity of air in flowing out into the reservoir must become of double volume, and the velocity there must consequently be 100 feet. In the same way the velocity in other portions of the tube could be determined by introducing a manometer and measuring the pressure at which the air passed the different portions. By repeating these experiments with tubes of the same diameter and different lengths, as well as with tubes of the same length and different diameters, the influence of length and diameter on the velocity of the air could be ascertained, and it could thus be finally determined as a function of the initial and final pressure, of the dimensions of the tubes, and of a constant depending on the nature of the inner surfaces of the tubes.

In the Appendix are given some of the numerous experiments made. These led to the following formulæ for the final velocity v , of the air in the tube, from which the initial velocity v'' , and in general the velocity v at any distance x from the beginning of the tube is measured; whence finally results the mean velocity $v' = \frac{v + v''}{2}$. Let l represent the length of the tube, d its inside diameter, h the pressure of the air on its entrance, h' its pressure

on exit, $h-h'$ the working pressure, and, lastly, a the above mentioned constant :

$$\text{The final velocity } v_f = a \cdot \frac{h-h_i}{h} \sqrt{\frac{d}{l}} \quad (1)$$

$$\text{The initial velocity } v_i = a \cdot h_i \frac{h-h_i}{h^2} \sqrt{\frac{d}{l}} \quad (2)$$

The velocity at the distance x from the beginning of the tube—

$$v = a \frac{(l-x) h_i + x h}{l} \times \frac{h-h_i}{h^2} \sqrt{\frac{d}{l}} \quad (3)$$

$$\text{The mean velocity } v' = a \times \frac{h^2 - h_i^2}{2 h^2} \sqrt{\frac{d}{l}} \quad (4)$$

The series of experiments proved these formulæ to be only approximate. The mean velocity of the air increases in reality more quickly than the square root of the diameter of the tube. This variation arises probably from the film of air attached by molecular attraction to the side of the tube, diminishing the section, which cannot be left out of consideration with narrow tubes. As the correction of this defect arising therefrom gives a greater velocity of air in wider tubes, and therefore produces better results in practice than calculation gives, it may be passed over.

The constant a occurring in the formula, and depending on the nature of the inner surface of the tube, is 15·950 according to the results of the experiments. If by means of this number the mean velocity of the air in a tube 13,000 ft. long and 3 in. in diameter is calculated for a difference of pressure of one atmosphere, we obtain—

With 1 atmosphere of pressure, therefore $h=2$ $h_i=1$, a mean velocity of 26'2 per second ;

With 1 atmosphere of vacuum, therefore $h=1$ $h_i=0$, a mean velocity of 35'0 per second ;

With $\frac{1}{2}$ atmosphere of pressure and $\frac{1}{2}$ atmosphere of vacuum, therefore $h=1\frac{1}{2}$ and $h_i=\frac{1}{2}$, a mean velocity of 31'1 per second.

It follows from what precedes that with long tubes of moderate size, and with practically applicable differences of pressure, it is possible to attain a sufficient velocity for the air in the tubes. If

the carrier is so constructed that it passes through the tube with a minimum of friction, the speed of the transmission of despatches almost coincides with that of the motion of the air.

The slight moment of inertia of the carrier, as well as the inertia of the air itself, may be left out of consideration, for both forces entirely vanish as compared with the friction of the air overcome in the tubes. The conditions would be quite different, however, if the carrier required a considerable force to move it forward in the tube. In this case so much more pressure would have to be exerted behind the carrier than before it, that the difference of pressure may compensate the frictional resistance of the air on the surface of the tube. This would, under otherwise similar conditions, bring about a decided diminution of speed. This disadvantage would occur specially with relatively narrow tubes. It is therefore necessary to employ carriers as far as possible without frictional resistance, and consequently to make them run on wheels of as large diameter as possible. The dimensions of the despatch tube are determined by this requirement. As the velocity increases only as the square root of the diameter of the tube (under similar conditions) and diminishes as the square root of the length, it is possible, without altering the ratio of pressure at the ends of the tube, to extend the transmission by pneumatic agency to such a degree, as the diameter of the tube can be increased in proportion to its length. With equal difference of pressure the carrier can be sent through a double length of tube of double diameter with equal velocity.

For the purpose in question, the forwarding of enclosed telegraph messages, a diameter of tube of 8 in. would suffice, as the envelopes are not usually more than 2 to 2½ in. wide.

For sending messages to and fro a single tube may serve, as an engine set up at the central station would produce a vacuum in one reservoir and store another with compressed air; and then one end of the tube could be put in connection with one or other reservoir, according as the carrier had to be received or despatched. Such an arrangement would, however, even setting aside the large dimensions to be given to the reservoirs, have only a low efficiency, and would not be at all fit for development. The necessity will soon arise for employing the same tube for pneumatic communication between several stations, and therefore for lengthening the

tube originally laid, and for converting the existing terminal station into a through station for the messages going further on. It is therefore desirable to make the arrangements such that they will also be available for the purpose of extension. It will therefore be advantageous to lay at once two tubes, one of which shall be employed for the despatch and the other for the receipt of messages. Should other stations be arranged later a rapid succession of despatches will become necessary to meet the expected important increase in the forwarding of messages. To make this possible, such an arrangement must be made that the tube from and to the central station shall be a circular conductor. Through this circuit system a permanent stream of air must be driven by the pump at the central station, which seizes on the carrier placed anywhere in the tube, and eventually drives it through the other stations back to the central station, unless previously received by some other station, as advised by telegraph signal. How this is performed will be explained later.

But if on these grounds it is desirable to employ a circular system, through which a continuous current of air should pass, there are other important considerations also in its favour. As is shown in formula 4, the mean velocity of the air depends on the factor $\frac{h^2 - h_1^2}{h^3}$, and therefore remains unchanged when h and h_1 , and therefore also their difference, are proportionately diminished. The work performed by the pump is, however, directly proportional to the density of the air to be compressed, and therefore diminishes proportionately as h . If, consequently, the circuit system is hermetically closed, and the apparatus so arranged that the mean density within the tube can be diminished at will, the work is diminished to the same extent.

APPENDIX.

THE experiments made by us to test the accuracy of the preceding formulæ are collected in the following tables.

The apparatus was so arranged in the above observations that air flowed from one reservoir directly connected with the air pump, the pressure of which was measured by a mercury manometer through tubes, and finally through the gas-meter into the

TABLE I.

DEPENDENCE OF VELOCITY ON PRESSURE.—ON ONE SIDE EXCESS PRESSURE,
ON THE OTHER ATMOSPHERIC PRESSURE.

$h-h_1$ in Centimetres.	$\frac{h-h_1}{h}$	Q. in Cubic Feet.	Velocity.	
			Observed.	Calculated.
1	2	3	4	5
16	0·174	0·47	22·6	22·0
18	0·192	0·51	24·6	24·3
20	0·208	0·55	26·6	26·2
22	0·225	0·59	28·6	28·4
24	0·240	0·64	30·5	30·2
26	0·255	0·67	32·2	32·1
28	0·270	0·71	34·0	34·0

atmosphere. The barometer was observed from time to time ; this remained constant at 760^{mm}. The lead tube used for the experiments had a length of 348 Prussian feet, and a diameter of $\frac{1}{4}$ Prussian inch. Column 1 gives the differences of pressure at the two ends of the tube ; col. 2 the ratio of this difference to the greater pressure ; col. 3 the quantity of air flowing through in a minute ; col. 4 and col. 5 the corresponding observed and calculated velocities in feet per second.

The last are calculated on the assumption that the velocities are in direct proportion to the differences of pressure, and in inverse proportion to the greater pressure. This assumption, even if not quite correct, is admissible without notable error within our requirements. This simple ratio is applicable because the theoretical

TABLE II.

DEPENDENCE OF VELOCITY ON PRESSURE.—PRESSURE ON ONE SIDE,
VACUUM ON THE OTHER.

No.	Pressure in Centimetres.	Quantity of Air passing through.		
		In the Middle.	At the End.	Calculated.
1	2	3	4	5
I.	±7	0·186	0·205	0·201
II.	±10	0·240	0·277	0·270
III.	±12	0·267	0·317	0·311
IV.	±16	0·313	0·396	0·396

values have to be multiplied by a variable experimental coefficient (diminishing with increase of pressure) to obtain the observed values.

As regards these experiments it may be observed :—The gas-meter was in the middle of the lead tube, and the pressure was measured in the middle and in the exhausted space. By continued pumping we were able to maintain the atmospheric pressure constant in the middle of the tube. For this case the measurements were then made, and as the law, that the pressure in a tube diminishes proportionally with the length was taken as correct, we calculated with the observed atmospheric pressure in the middle of the tube an excess pressure in the space under pressure equal to the measured vacuum in the exhausted space. Column 3 gives the quantities measured at the middle of the tube under atmospheric pressure ; column 4 the outflow into the vacuum reservoir calculated according to Mariotte's law ; column 5 contains the calculated quantities of outflow in which each number is calculated from the next following.

TABLE III.
DEPENDENCE OF VELOCITY ON LENGTH OF TUBE.

I. No.	II. $h - h_r$ in Inches.	III. Diameter in Inches.	IV. Length in Feet.	V. Q. in Cubic Feet.	VI. Velocity.		VII.
					Observed.	Calculated.	
1	6	0.25	112	0.7	34.3		34.3
2	84	0.8	39.2		39.6
3	56	1.0	49.0		48.7
4	28	1.4	68.6		68.6

TABLE IV.
DEPENDENCE OF VELOCITY ON DIAMETER OF TUBE.

No.	$h - h_r$	Length.	Diameter.	Q.	Velocity.	
					Observed.	Calculated.
1	12	100'	6.75	0.860	42.1	42.1
2	5.20	0.450	36.4	36.9
3	3.25	0.185	27.0	26.0
4	10	...	6.75	0.810	39.6	39.6
5	5.20	0.401	32.2	34.6
6	3.25	0.161	23.4	24.4

REMARKS ON TABLES III. AND IV.—The length and diameter of the tubes were measured directly. In Table III., column 2 gives the difference $h - h_1$ in inches of mercury, whilst in Table IV. $h - h_1$ is given in centimetres of mercury. The numbers contained in column 7 of Table III. are all calculated from the first velocity 34.4, on the assumption that the velocities vary inversely as the square roots of the lengths. The numbers contained in column 7 of Table IV. are calculated, Nos. 2 and 3 from No. 1, and Nos. 5 and 6 from No. 4, and give the law that the velocity is in direct proportion to the square root of the diameter of the tube.

RESULTS.—We hence obtain the value—

$$v_1 = a \frac{h - h_1}{h} \sqrt{\frac{d}{l}} \quad . \quad . \quad . \quad (1)$$

for the velocity of outflow of the air from a cylindrical tube, in which l is the length of the tube, d its diameter, h the greater and h_1 the less pressure, and a is a constant. If the value of the constant is calculated from this formula with the help of the data given in the previous table, we obtain—

$$a = 15950 \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

By the use of Mariotte's law we can arrive at the velocity of inflow from that of outflow. The mean of these two then gives the mean velocity of the air in the tube, which is what we want. This mean velocity is found to be—

$$v_1 = a \frac{h^2 - h_1^2}{2 h^3} \sqrt{\frac{d}{l}} \quad . \quad . \quad . \quad (3)$$

According to formula 2, the mean velocities of the air in tubes of 13,000 feet length (twice the distance between the central and proposed end station of the projected line) of different diameters, and assuming—

- a. An atmosphere of pressure.
- b. An atmosphere of vacuum.
- c. Half an atmosphere of pressure and half an atmosphere of vacuum, are the following :—

Diameter in Inches.	Mean Velocity.		
	1 Atm. Excess pressure.	1 Atm. Vacuum.	$\frac{1}{2}$ Atm. Pressure. $\frac{1}{2}$ Atm. Vacuum.
2 $\frac{1}{2}$	23·9	32·0	28·4
3	26·2	35·0	31·1
3 $\frac{1}{2}$	28·3	37·8	33·6
4	30·3	40·4	35·9

METHOD FOR CONTINUOUS OBSERVATIONS OF THE TEMPERATURE OF THE SEA WHEN SOUNDING.*

MR. EHRENBURG brought forward the following communication which he had obtained upon request from Dr. Werner Siemens regarding a method suggested by himself and his brother William in London for observing the temperature of the sea continuously when taking soundings.

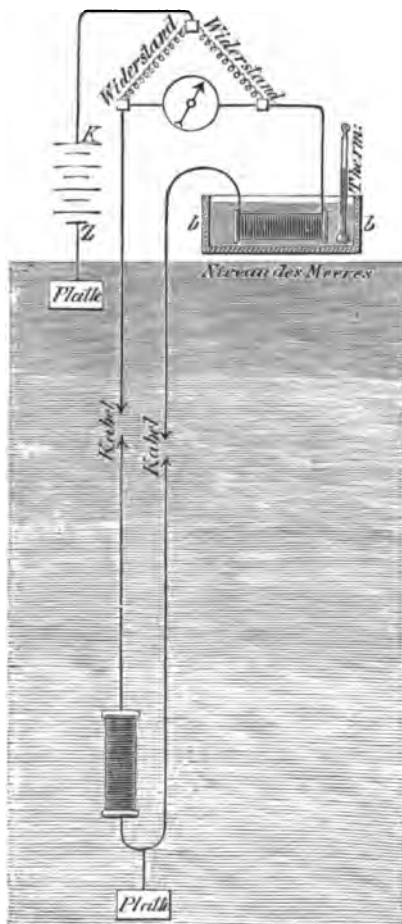
The method depends on the fact that the resistance of metals depends on their temperature. By measuring the resistance of an insulated coil of wire immersed in sea water, the temperature of the latter can be deduced if the resistance of the coil is known at a definite temperature. The resistance of copper varies 0·394 per cent. per degree Centigrade.

This method, however, suffers from the defect that the ends of the resistance coils must be connected with the ship by means of wires of very little resistance, which therefore must be thick, so that the variation in the resistance of the leading wires caused by change of temperature may cause no sensible error. Exact determinations of resistance also require very good apparatus and experimental dexterity. We have, therefore, more recently so far changed the method that no measurements of resistance are necessary, and the temperature of the sea can be read on board ship by the usual mercury thermometer. This is effected by

* Monthly Report of the Berlin Academy of Sciences. Poggendorff's Ann. d. Phys. u. Chem., 1866, Vol. CXXIX. p. 647.

combining the resistance coil placed at the end of the double-cored cable, which serves as a sounding line with three other

Fig. 27.



exactly similar resistance coils placed on board the ship, and with a galvanometer having an astatic needle to form a so-called Wheatstone bridge as shown in the accompanying figure. One of the resistance coils on board ship is placed in a water or oil bath, which can be cooled or heated as desired. If the temperature of this bath, and consequently that of the coil immersed in it, is different from that of the water surrounding the resistance coil sunk in the sea, a current would pass through the galvanometer, and the needle be deflected. If there is no deflection, the temperature of the sea and bath are exactly alike, and hence the thermometer placed in the latter will give the temperature of the sea. As one leading wire is in the branch of the sunken coil, and the other in that of the coil in the bath,

both are equally warmed or cooled by the surrounding sea water, and their disturbing influence is entirely eliminated. Very thin leading wires can hence be employed, which is of great practical importance.

The sinker to be released on touching bottom, and the arrangement for hauling up samples of the bottom, remain unchanged. The substitution for the hemp line formerly used of a thin double-cored cable served with hemp certainly makes the apparatus considerably dearer, and necessitates the arrangement of a special apparatus for unrolling and coiling in the cables ; but, on the other hand, the great strength of such a cable prevents the frequent loss of the line employed.

ON THE CONVERSION OF MECHANICAL ENERGY INTO ELECTRIC CURRENT WITHOUT PERMANENT MAGNETS.*

WHEN two parallel wires which form part of the closed circuit of a galvanic battery are brought near to or removed from one another, an increase or diminution of the current of the battery is observed, according as the motion is in the direction of the force which the currents exert on one another or in the opposite direction. The same phenomenon occurs with increased intensity when the poles of two electromagnets, the convolutions of which form part of the same closed circuit, are brought near to or removed from one another. If the direction of the current is reversed in one wire at the moment of the closest approach and furthest removal, as is done by mechanical means with electrodynamic rotatory apparatus and electromagnetic machines, a continual diminution of the strength of the current occurs so soon as the apparatus is set in motion. This weakening of the battery current by means of the counter current which is generated by the motion in the direction of the moving forces is so considerable, that it explains why electromagnetic motors could not be driven successfully by means of galvanic batteries. If such a machine be turned in the opposite direction by means of an external force, the battery current will be increased by the induced current now flowing in the same direction. As this increase of the current

* Monthly Report of the Berlin Academy of Sciences for 17 January, 1867.

also causes an increase of the magnetism of the electromagnet, and consequently also a strengthening of the following induced current, the current of the battery increases rapidly to such a point that the battery itself may be disconnected without any observable diminution of the current. If the rotation is stopped, the current naturally disappears and the stationary electromagnet loses its magnetism. But the small quantity of magnetism which always remains in the softest iron suffices on renewing the rotation to generate again a progressive increase of the current in the circuit. Thus a weak current from a battery requires only to be passed once for all through the coils of the fixed electromagnet to render the apparatus always serviceable. The direction of the current which the apparatus produces is dependent on the polarity of the remanent magnetism. It is altered by means of a momentary current in the opposite direction passed through the coils of the fixed magnet; this suffices to give an opposite direction also to all the subsequent powerful currents generated by rotation.

The action described must occur indeed with every electromagnetic machine which is based on the attraction and repulsion of electromagnets, the coils of which are included in its own circuit; but still special considerations are required to construct such electrodynamic machines of great power. The fixed magnet encircled by the commutated currents must have sufficient magnetic inertia also to retain unimpaired the highest amount of magnetism induced in it during the change of current, and the opposite poles of both magnets must be so arranged that the fixed magnet has always a closed magnetic iron circuit whilst the moving one is revolving. These conditions are best fulfilled by the form of magneto-machine proposed by me a long time ago, and since much used by myself and others. The rotating magnet in this consists of an iron cylinder revolving on its axis, provided with two slots opposite to one another and parallel to the axis, which receive the insulated coil of wire. The poles of a great number of steel magnets, or in the case in question the poles of the fixed electromagnet, surround the periphery of this iron cylinder throughout its whole length with the least possible clearance.

With the help of a machine arranged in this way, and driven at a sufficiently high speed, when the relations of the separate portions are properly determined, and the commutator is properly ad-

justed, currents of such strength may be generated in closed circuits of small external resistance, that the wires surrounding the electromagnets are heated by them in a short time to such a temperature that the covering of the wire becomes charred. When the machine is regularly used this risk may be avoided by the insertion of resistances, or by moderating the velocity of rotation. Whilst the output of magneto-electric machines does not increase in the same proportion as their dimensions, the converse takes place in those described. The cause of this is that the force of the steel magnets increases in a much less ratio than the mass of the steel employed in their construction, and that the magnetic power of a large number of small steel magnets cannot be concentrated in a small polar surface without weakening the action of such magnets considerably or quite demagnetizing some of them. Magneto-machines with steel magnets are therefore not adapted for use where it is required to produce very strong continuous currents. The construction of such powerful magneto-electric machines has indeed often been tried, and such powerful currents have been produced with them, that they gave an intense electric light, but these machines were obliged to be made of enormous dimensions and were therefore very costly, moreover the steel magnets soon lost the greatest part of their magnetism, and the machines their original force.

Lately, Wilde of Birmingham has considerably increased the efficiency of magneto-electric machines by combining in one machine two magneto-machines of my above described construction. He provides the larger of these machines with an electromagnet in the place of a steel magnet, and uses the other to effect the continuous magnetization of this electromagnet. As the electromagnet is stronger than the steel magnet it replaces, the current produced must be increased by this combination in at least the same proportion.

It can easily be perceived, that by means of this combination Wilde has considerably diminished the above mentioned defects of the steel magnet machine. Setting aside the inconvenience of employing two inductors at the same time to produce one current, his apparatus is dependent on the uncertain performance of the steel magnet.

By means of the method employed, electric currents can be pro-

duced in a cheap and simple manner, wherever mechanical agency is available. This circumstance will be of considerable importance in many departments of the Arts.

DEAD-BEAT NON-ASTATIC MAGNETS.

(From a paper by PROFESSOR DU BOIS REYMOND on *dead-beat motion of damped magnets*.*)

IN speaking of the experimental conditions under which the motion of damped magnets becomes dead-beat † I said in the first of the memoirs referred to below : "Another way under otherwise similar conditions to make $r=0$ or real would be the reduction of the moment of inertia M . It follows from the very nature of things, that without special contrivances this cannot be done constantly, and not with apparatus already manufactured. But the smaller M is, the thinner, for instance, a magnet mirror is of otherwise similar figure, with so much less astaticism does its motion become dead-beat." ‡ I have since frequently thought whether one might not succeed in making its motion dead-beat by reduction of the moment of inertia alone without making the needle astatic.

I therefore set about hanging magnets of the thinnest steel plate in silver dampers, in doing which the only difficulty was that such mirrors give no image in the telescope, whilst combination with even the lightest glass mirrors increases the moment of inertia too much. §

In the meantime this problem has been solved by my friend Dr. W. Siemens in a manner which appears all the more ingenious the more strange the solution seems at first sight. Dr. Siemens has made dead-beat swinging magnets without astaticising them, which, although the reduction of the moment of inertia has been kept in

* Monthly Report of the Berlin Academy of Sciences, 1873, p. 748.

† See Monthly Report of the Berlin Acad., 1869, p. 807 ; 1870, p. 537.

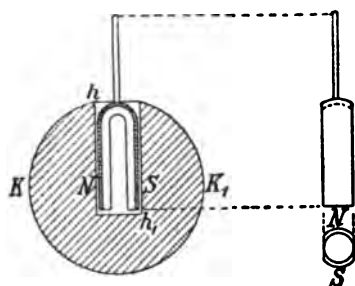
‡ Idem, p. 836.

§ Archives des Sciences Physiques et Naturelles, N.P., 1872, Vol. XLV., p. 92.

view, are yet powerful enough to allow of their being connected with a mirror without detriment. He has been so good as to permit me to communicate this construction to the Academy in his name.

It is shown half-size in Fig. 27A. K K' is a copper sphere shown

Fig. 27A.



in section, in which is bored a cylindrical cavity $h h'$, the axis of which corresponds with the vertical diameter of the sphere, and the axis around which the magnet let into the hollow rotates. The best idea of the latter may be formed if one conceives a thimble or steel bell with two cuts made parallel to one another and to the axis and at equal distance from the latter. There remains then a loop, which the principal figure shews in a section taken parallel to the axis, the neighbouring figure in a section perpendicular to the former, and which is seen in ground plan from below. A stem is fixed in the centre of the loop, in the continuation of its axis, by which it is suspended centrally in the cylindrical hollow of the damper, and to the top of which the mirror is fixed; magnetically speaking, the loop represents a horse-shoe, the poles of which lie opposite one another in its legs N S. Dr. Siemens calls such magnets bell magnets.*

There is obtained by this arrangement: 1. A greater intensity

* In another sense be it noted than Mr. Wiedemann uses this expression. (Theory of Galvanism and Electromagnetism, 2nd ed., Vol. II., Brunswick, 1873, p. 483.) An account of the bell magnet is to be found in Zetsche; short communications on the new telegraph apparatus exhibited by Siemens and Halske in Vienna in 1873, in Schlömilch's, Kahl's and Cantor's Zeitschrift für Math. und Phys., 1873, p. 427.

of magnetization by the horse-shoe form. 2. A relatively smaller moment of inertia. 3. A greater proximity of the poles to the damping metallic mass. 4. Independence of damping on deflection.

The consequence of this arrangement is that the magnet not only swings dead-beat without being astatic, but also that ϵ is considerably greater than n . To obtain the well known advantageous limiting condition $\epsilon = n$, the Hany bar must either be used in the opposite direction, or the magnet must be raised somewhat above the damper. The Siemens arrangement fulfils even more correctly than mine Gauss's original conception. Its sensitiveness, when the bell magnet is used as a galvanometer needle, leaves nothing to be desired. The stability against concussions from passing carriages, etc., is specially great. The period of bell magnets is not only very small in proportion to their mass, but also when taken absolutely, even smaller indeed than that of my light mirror I., for with a sample which I tested it amounted to only 3" in the case of the limits of the scale, which indeed enclosed only a very small angle.

CAPILLARY GALVANOSCOPE FOR THE MEASUREMENT OF RESISTANCE IN SUBMARINE CABLES.*

MR. SIEMENS brought before the Section a capillary galvanoscope constructed by him, which is specially intended to render possible the measurement of resistances of submarine cables on tossing ships.

The instrument is a modification of Lippmann's capillary electrometer. It consists of two wide glass tubes fixed perpendicularly in a small base at about 3^{cm} distance. Directly above the surface of the base they are connected by means of a thin glass tube slightly curved upwards, of about $\frac{1}{2}$ ^{mm} internal diameter. A scale with millimetre divisions is fixed on the base below this capillary

* Monthly Report of the Berlin Academy of Sciences, 1874, p. 157.

connecting tube. The two wide vertical glass tubes, as well as the capillary connecting tube, are so filled with pure mercury free from air that in the middle of the capillary tube, the thread of mercury is broken by a thread of sulphuric acid a few millimetres in length. With a little practice the filling is easily done without admitting air.

The above described arrangement has decided advantages over Lippmann's, in which the capillary tube is half filled with mercury and half with sulphuric acid. In Lippmann's arrangement the shifting of the meniscus which is brought about by the alteration of the capillary constant connected with the polarization is much less, as by lengthening or shortening the mercury column in the vertical capillary tube a rapidly increasing opposing force is called forth, which limits the shifting. With the arrangement described, on the contrary, no noticeable alteration in the level of the mercury cups occurs in the wide vertical tubes. It is therefore necessary to bend the capillary connecting tube slightly up, so that even the weakest current may not expel the drop of sulphuric acid from the tube. Should this nevertheless take place when too strong currents are used, the drop of sulphuric acid clings by adhesion to the mouth of the capillary tube and returns on reversal of the current. A second very important advantage of the arrangement described consists in the mercury cups limiting the sulphuric acid threads becoming both polarized, by which the pushing force is increased, and is equally strong for both directions of the current. Finally, the resistance to motion brought about by the adhesion of the sulphuric acid to the walls of the capillary tube is much smaller with the described construction on account of the shortness of the thread of sulphuric acid.

If the instrument is to be made insensible to the motion of the ship, the perpendicular glass tubes are provided with auxiliary tubes, which are curved inwards, so that their mercury cups lie near together over the middle of the capillary tube.

One defect which renders the use of this capillary galvanoscope difficult is that the thread of sulphuric acid returns to its place of rest only very slowly after deflection by a current, even when care is taken to depolarize the mercury cups by metallicly connecting the two mercury columns. If however the same quantity of electricity which caused the polarization is sent through the instrument

in the opposite direction, the thread returns quickly and exactly to its original position. This is easily effected by inserting a condenser in the circuit, the forward motion taking place on the charge and the return motion on its discharge. The instrument can only be employed for exact measurements when very great resistances and considerable electromotive forces come into play. If the electromotive force of the battery is not at least $\frac{1}{10}$ Daniell no observable motion of the thread takes place. Notwithstanding this, the instrument can be usefully employed in many cases, especially when very great resistances are included in the circuit. These make the motion of the thread of sulphuric acid slow, but are altogether without influence on the extent of the motion.

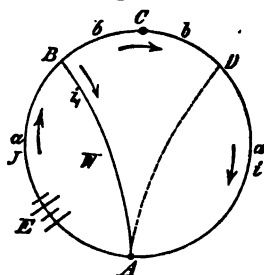
DIRECT MEASUREMENT OF THE RESISTANCE OF GALVANIC BATTERIES.*

OHM's method of repeated measurement of currents by the insertion of known resistances gives, as is known, the necessary data for calculating or comparing the three constants of the galvanic battery. As the measurement of currents necessitates the employment of very exact measuring instruments, and the method is, besides, very tedious and occupies much time when it is to give anything like satisfactory results, it is very desirable to endeavour to determine the separate constants of the battery by direct measurement. This has succeeded in a very perfect degree as regards the measurement of the resistance of those parts of the battery where there is no E. M. F. at work, with the aid of the branch-current system known by the name of the Wheatstone bridge. The comparison of the E. M. F. of two batteries has been brought to a high degree of perfection by means of Mr. Poggenдорff's compensation method, although it still has the great defect that the E. M. F. are not compared under similar conditions, but are so compared that the one battery is on closed circuit of definite resistance, and therefore active, whilst no current passes through

* Poggenдорff's *Annalen d. Phys. u. Chemie*, Jubilee Vol. 1874, p. 445.

the other. In recent times Paalzow, Beetz and others have also proposed methods of determining the internal resistance of batteries in action, with the assistance of Dubois' perfected Poggendorff compensation method. Although it is not to be denied that in this way the problem of determining the resistance of galvanic batteries without the measurement of the current is solved in principle, yet these indirect methods have very serious defects. The desired resistance of the battery appears in this method as the result of calculation, in which are introduced, besides the known interpolated resistances, the resistance of the comparison battery and the ratio of the E. M. F. of both batteries, since, as is now known and confirmed by the experiments described later on, neither the E. M. F. nor the resistance of a galvanic cell is really

Fig. 28.



constant, but both vary with the relative strength of the current, consequently with the degree of activity of the unit of surface of the cell; therefore the introduction of the E. M. F. of the battery into the calculation must necessarily essentially affect the result obtained. Setting aside this theoretical defect, the methods founded on the compensation principle are too minute and troublesome, especially for technical use.

My method of direct determination of the internal resistance of the battery depends, like Wheatstone's method of measuring resistance, on a law of derived circuits.*

A B C D (Fig. 28) represents in the following diagram the

* I have already described this method before the Society of Telegraph Engineers in London, at their meeting on the 11th December, 1872.

circuit of the battery E, the internal resistance of which is to be determined; C represents the point of bisection of the resistance of the whole circuit, so that $AB = CD = a$; further $BC = CD = b$. A shunt of resistance w is connected to A, and can be joined up to either the point B or D by means of a commutator. Thus, according to Kirchhoff's laws—

$$(a - b) J + (a + b) i = E \quad . \quad . \quad . \quad . \quad (I.)$$

$$w i - (a + b) i = 0 \quad . \quad . \quad . \quad . \quad (\text{II.})$$

$$i_1 + i_2 = J \quad . \quad . \quad . \quad . \quad (III.)$$

and when J and i are eliminated from these three equations—

$$i = \frac{E w}{a^2 + 2 a w - b^2}.$$

In this expression for the strength of the current i in the part of the main circuit, which has no E. M. F., the distance b of the shunt from the dividing point of the total resistance occurs only as the square; i therefore remains unchanged if $-b$ is put in place of b , or if the shunt is joined to the point D instead of B. It is easy to make a contrivance to measure the internal resistance of a battery by means of this law of derived circuits. If $BCD = 2b$ is any resistance, the amount of which had best be between once and twice the resistance of the battery; if, further, $AD = a - b$ is the coil of a galvanometer and w a resistance coil, one end of which is joined up to one pole of the battery and to one end of the galvanometer wire, whilst the other end is joined up in quick succession with the point B or D, then it is only necessary to add or withdraw resistance between the free pole of the battery and the point B by means of a rheostat until the needle of the galvanometer remains unaffected, whether the connection is established at the point B or D of the circuit. The internal resistance of the battery is then equal to that of the galvanometer, less the resistance inserted to obtain equilibrium. I append a series of experiments which Dr. Frölich has made in my laboratory by this method, proving its reliability and practicability.

Numerous determinations with various forms of Daniell cells were made according to Dr. Werner Siemens's method of measuring the resistance of batteries. It was found that in order to obtain constant and comparable results the following conditions had to be observed :—

1. The current during the whole measurement must remain quite constant.

2. The proportion of the current in the battery branch must be maintained constant within certain limits.

The carrying out of the first condition is obvious and easily obtained by carefully setting up the batteries and joining them up some time before the measurement, so that the strength of the current may have the same value as in the subsequent experiments.

The necessity of the second condition was also to be expected, for the surface resistance which particularly comes in here is a function of the strength of the current. If, therefore, different constant cells are to be compared as regards their resistance, whether singly or together, in order to obtain correct results, the resistances have to be so arranged in the circuit that both the current strengths in the battery branch, which correspond to the two positions in the branch C (in Fig. 80), may always have the same value in the separate measurements.

As it is troublesome to carry out this condition strictly, it became requisite to determine experimentally to what extent the strengths of the current might differ from one another without affecting the results of the measurements ; further, the certainty of the method should be proved.

The safest test of the latter consists in measuring cells individually as regards their resistance, then to connect them up in series in various groups, and again to measure the resistance. The resistances of each group must then agree with the sum of the resistances of the single cells.

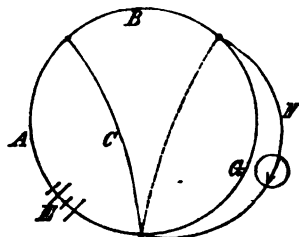
This test was made with five small Daniell cells, the resistances of which were first determined separately and then in groups of 2, 3, 4 and 5 ; whilst both current strengths in the battery branch A were made almost equal in the different measurements.

There was an almost perfect agreement in the results ; the deviations are almost of the same order as the errors of observation, and it showed, further, that to maintain the strength of the current in the battery branch equal a somewhat wide range is requisite.

As regards the sensitiveness of the method, it may be laid down generally as a rule that the maximum is reached when in the battery branch A there is no resistance beyond that of the cell, and that in the branches B and C the resistances have about the same values as in the battery branch.

The strength of the current in the battery branch was measured

Fig. 29.



with a tangent galvanometer, that in the branch G with a delicate mirror galvanometer. The latter was, however, placed in a shunt *N* to the branch G (Fig. 29), in which the resistance was maintained so great in comparison with that in G that for the calculation of the resistance of the cell the shunt *N* may be left out of consideration.

A Poggendorff's wippe was used as commutator. The sensitiveness of the measurement, that is the difference between the two currents, when 0.1 Siemens unit more or less was inserted in G amounted, with the measurement of the separate cells, to 2^{mm} deflection on the mirror galvanometer ; with five cells in series to 0.5^{mm}.

The following table contains the measurements of the resistances of the various groups of the cells. The deflections of the mirror galvanometer are added to show that the current remained of about the same strength also in the galvanometer branch.

This is certainly immaterial ; however, the error which arose from variations of the zero point of the needle would thus be alike in all measurements.

Elements.	G.	Reading on Mirror Gal- vanometer.	Strength of Current in Battery Branch.	
			Position 1.	Position 2.
I.	Units. 2·00	mm. 290	0·312	0·198
II.	2·60	300	0·270	0·176
III.	1·75	285	0·333	0·194
IV.	1·60	274	0·340	0·209
V.	1·40	265	0·378	0·215
I. + II.	4·20	294	0·291	0·176
II. + III.	4·00	290·5	0·291	0·185
III. + IV.	3·00	267	0·361	0·194
IV. + V.	2·60	252	0·366	0·209
I. + II. + III.	5·55	289	0·306	0·198
II. + III. + IV.	5·20	285	0·325	0·190
III. + IV. + V.	4·00	263	0·376	0·224
I. + II. + III. + IV.	6·80	276	0·310	0·190
II. + III. + IV. + V.	6·20	268	0·329	0·190
I. + II. + III. + IV. + V.	7·80	273	0·327	0·224

In all the measurements the resistance of the wire in A amounted to 0·40 unit (leads to the cells) ; the resistances in the branches B C N were as follows :—

	B.	C.	N.
	Units.	Units.	Units.
Cells separately . . .	3	3	1500
„ 2 in series . . .	7	7	3000
„ 3 „ . . .	10	10	4500
„ 4 „ . . .	15	15	6000
„ 5 „ . . .	18	18	7500

From these values are reckoned the resistances of the cells by simply deducting the resistance of the wire in A from G. We then obtain—

Elements.	Resistance.	Resistance Calculated.	Error.
I.	Units. 1·60	Units.	Units.
II.	2·20		
III.	1·35		
IV.	1·20		
V.	1·00		
I. + II.	3·80	3·80	0·00
II. + III.	3·60	3·55	- 0·05
III. + IV.	2·60	2·55	- 0·05
IV. + V.	2·20	2·20	0·00
I. + II. + III.	5·15	5·15	0·00
II. + III. + IV.	4·80	4·75	- 0·05
III. + IV. + V.	3·60	3·55	- 0·05
I. + II. + III. + IV.	6·40	6·35	- 0·05
II. + III. + IV. + V.	5·80	5·75	- 0·05
I. + II. + III. - IV. - V.	7·40	7·35	- 0·05

The groups of 2, 3, &c., are calculated from the values obtained for the separate cells. The result simply shows that the resistance of cell III. was measured 0·05 unit too little.

The variations of current strength in the battery branch A are the following :—

In position 1 of branch C, from ·270 to ·378.

„ 2 „ from ·176 to ·244.

It follows in general from these measurements that the proposed method, when properly applied, gives thoroughly reliable results ; that there is no need, however, to take special pains to carry out the second of the above-named conditions ; and that the method has, therefore, the further advantage of easy and rapid application.

INAUGURAL SPEECH OF DR. SIEMENS AND REPLY
OF PROF. DU BOIS REYMOND, SECRETARY OF
THE PHYSICO-MATHEMATICAL SECTION.*

THE Academy, by admitting me among its members, has done me an honour which I have not sought, and was not entitled to expect. Up to now only those savants were called to these chairs rendered venerable by the high scientific attainments of former as well as present occupants, whose sole vocation was science, and who had successfully devoted the whole of their intellectual power to it. And there were strong reasons for adhering to this custom. The universal homage which the world renders to German science is due to the well-grounded fame of the thoroughness of its work, to the depths of its investigations, and specially to the strict demand for a fundamental and systematic preparation for the scientific calling. This inspires a young man with the love of science, and strengthens him to carry out his resolution of henceforth devoting his life to it. This it is which has preserved to German science the purity of scientific tendency, which forms its highest honour. The German scientist does not ask whether the problem the solution of which he is undertaking, whether the research to which he is devoting himself, will bring immediate advantage to himself or others, it is the pure unselfish love of science which lays down his problem for him, it is the ardent desire for knowledge which incites him to devote the whole of his mind to carry it through, often under the afflicting cares of life, until he is exhausted. The consciousness of having increased the one genuine treasure of mankind, its store of knowledge, is reward enough for him, and his ambition is satisfied if his name is for ever associated with the discovery of some new truth, of a new scientific fact or deduction.

The Academy by choosing me, has deviated from the system which has accomplished such a success. It has considered a man worthy to enter its ranks whose professional activity appertained

* Read at the public meeting of the Royal Academy of Science of Berlin on the 2nd July, 1874.

neither to science itself, nor to scientific teaching nearly allied to it, to whom also it was not permitted, as the disciple of some great master to climb up, under safe guidance, to the shining heights of the science of the day, and then on this solid foundation of the intellectual work of the whole human race, collected during a long series of centuries, to be able to co-operate in raising them yet higher, with comparative ease.

I am not presumptuous enough to think that the contributions which I have made to pure science have alone been decisive for this. I believe, and I take comfort in the conviction, that more weighty considerations have influenced the Academy. I account for it by the circumstance that thanks to better education at schools and to the greater development of mental intercourse, which now quickly makes every new thought and every new scientific fact unalienable for the future general good of mankind—scientific knowledge and method are now no longer confined to the narrow circle of the professional scientist, but exert their animating and fructifying influences over larger circles of the community. They have been introduced as essential factors into the teaching profession, into official life, into the manufactures, into agriculture, even into most of the trades. Thousands of co-workers in science have thus sprung up, who certainly for the most part do not stand upon a height of knowledge affording a wide survey, yet knowing their specialty thoroughly, and endeavouring to extend it with the assistance of the scientific knowledge they have acquired, arrive everywhere at the limits of our present knowledge. The knowledge of new facts, of hitherto unknown phenomena, flows back thence to science in living streams. Yet it is not alone in the interest of science to tread in close connection with the application of the results of its investigation to practical life, because it richly returns what it receives; it is also for it a command of duty. For what gives science its higher sanction, what alone gives it a claim to the grateful affection and reverence of the people, is that it does not exist on its own account, for the satisfaction of the desire for knowledge of the restricted number of its professors, but that its task is to increase the store of knowledge and power of the human race, and to supply it thereby with a higher grade of culture. It is as it were the nervous system which runs through the organism of human culture, which

generates new life in its finest, hardly visible, ramifications, and not only increases thereby the ideal good of mankind, but lightens the hard fight for material existence by pressing into its service the still unknown slumbering forces of nature.

To this last object of scientific activity my exertions were always directed in my profession of the application of science. Unfortunately, it left me but little leisure for purely scientific investigations, to which I always felt specially attracted. My problems were generally prescribed by the demands of my profession, because the filling up of scientific voids which I met with presented itself as a technical necessity. I will only here mention cursorily my method of measuring high velocities by means of electric sparks, the discovery of the electrostatic charge of telegraphic conductors and its laws, the deduction of methods and formulæ for testing underground and submarine cables, as well as for determining the position of faults occurring in their insulation, my experimental observations on electrostatic induction, and the retardation of the electric current thereby, the conception and realization of a reproducible basis of measurement for electrical resistance, the proof of the heating of the dielectric of a condenser by sudden discharge, the discovery and explanation of the dynamo electric machine. I think I may also claim that many of my technical contributions are not without scientific value, among which I may mention the differential regulator, the manufacture of insulated conductors by pressing gutta-percha around them, telegraphic duplex, diplex, induction and automatic recording instruments, the ozone apparatus and measuring instruments of different kinds. I had the honour of seeing these recognized by receiving from the Berlin University the distinction of Doctor of Philosophy *honoris causa*. I cannot omit at this point thankfully to draw attention to the circumstance, that the friendly sympathy with which many of the elder members of this Academy always followed my efforts, as well as the ties of friendship which bind me to many of the younger, essentially combined to keep alive in me the love of science during my long technical career. It is true that I have seldom had leisure to prosecute with scientific consistency the new phenomena I met with beyond the limits of technical necessity, and also in future my professional labours will prevent me from altogether obeying my scientific inclinations.

But the Academy, by its choice of me among its members, has associated inclination with duty, a reminder which tends specially to influence one in the State of Frederick the Great, which also is not likely to remain without influence upon me.

Here followed Mr. Virchow's speech.

After the speeches of the newly-elected members to the Physical Mathematical Section of the Academy, Messrs. Siemens and Virchow, Prof. du Bois Reymond replied as secretary of the section, as follows :—

Your admission into the Academy, my dear Siemens, and yours Mr. Virchow, coincide not only as regards time, but also in many other points. Neither belongs properly to the usual occurrences in the life of our corporation. As a rule it fills up the voids which fate makes in its circle with younger forces, the rich development of which in the future it considers certain. It not seldom incorporates into itself men already mature and universally known, but this generally occurs on their coming to reside in Berlin. The names of Siemens and Virchow on the contrary have long been prominent ornaments in scientific Berlin. If there is anything which would surprise outsiders in this day's event, it would be that it has only happened to-day.

But the merits with which the world is accustomed to connect both names are partly of a kind to which Academies are naturally somewhat foreign : and because their dazzling lustre concealed the academical nucleus contained in it, they assisted in a wonderful way to retard this day rather than to hasten it on.

The practical application of science, making it serviceable for technical purposes, for which you, my dear Siemens, have done so much, lies outside the circle of our occupations. So far as this application assures wealth, power and consideration to him who successfully devotes himself to it, it is left to itself without risk of damage, and needs no status conferred by the State. Force and means and encouraging sympathy will never fail it. The development of industry for a century past, to which the learned societies have but slightly contributed directly, sufficiently proves this. In any case the passing of a good patent law should be of more service to industry than an immediate share of the Academy, in the solution of industrial problems, indeed a near lying example proves that, to succeed fully, industry does not even require this help.

Benjamin Franklin, one of the first apostles of utilitarianism, called mankind the machine-making animal. Hardly a century has since passed, and we proudly add, he is the animal who travels by steam, writes with lightning, and paints with the sun's rays. The systematic utilization of the stores of nature, the methodical subjection of its forces, are incontestably one of the highest objects in which mankind can engage, and we now approach this object with a safety and constancy, which justifies almost every hope, and causes man to appear more God-like than ever before. For under our present conditions, to make the sum of our well-being and enjoyments a maximum, and of our sufferings and privations a minimum, is an undertaking similar to that, which according to Leibnitz, in whose name we are here assembled, appeared to God himself at the creation of the world.

But man does not live by bread alone, and may ask with Novalis, which is more practical, to give man bread or an idea? After the sense of beauty is satisfied, which, according to Darwin, the bird shares with us, there is still in man one instinct, which, like speech, belongs to him alone of all living beings. The word, *Why?* which comes to us from the lips of unlearned children, as thousands of years ago it sounded from those oriental philosophers, is amongst all the words of the human language the word most suitable to man. The desire to ascertain satisfactory reasons is, so to speak, the pinnacle of what matter can do when it is built up into the brain and produces consciousness.

The appeasing of this desire, the satisfaction of the instinct seeking the cause of things, is the remote height, where the academic spirit tarries, but without some preparation, would soon be isolated. For he who would only search out real truth does not require to look about to know, that only a few go his way. Science does not seek after worldly goods, and scientific ambition is more a sign of talent.

Hence the Academy exists for the building up of scientific knowledge on its own account. That no democratic or oligarchic commonwealth ever founded an Academy, throws a peculiar light on the spirit of the different forms of government.

Sprung from the idealistically disposed Renaissance, the Academies in the realism now surrounding them stand out almost as heterogeneous creations. It is also inevitable that their standpoint

should be somewhat altered in the course of time. But by appropriating such a scientific form as yours, my dear Siemens, no Academy need be untrue to the laws of its foundation.

Yours is the talent of mechanical discovery, which primitive people not improperly described as divine, and the cultivation of which constitutes the ascendancy of modern culture. Without having yourself worked with your hands in practical mechanics, you have reached the highest point in that Art as creative and organizing head. With clear view and daring mind you soon grasped the great practical problems of electric telegraphy, and thus secured to Germany an advantage which Gauss, Wilhelm Weber, and Steinheil could not have procured for it. Long before the newly awakened German genius dispelled in Parliament and on the battle field the scornful prejudice that we are a nation of dreamers, yours and our Halske's apparatus, at each of the world's great exhibitions, forced from the jealous foreigner a wondering acknowledgment of what German science and German industry are able to accomplish. Your labours were for electricity what Fraunhofer's were for light, and you are the James Watt of electromagnetism. Now you rule over a world which you created. Your telegraph lines surround the globe. Your cable ships navigate the ocean. Under the tents of nomads using bows and arrows, through whose pasture grounds your messages pass, your name is mentioned with superstitious awe.

But it was less this sort of success, which has won you such a position in life and such far-reaching renown, that opened to you the doors of the Academy. But that from such a height, as prince of technists, holding in your hands the threads of numerous combinations, revolving hundreds of plans in your brain, you exist in the very soul of German learning in the noblest sense of the word born to what you were not brought up; that whenever the cares of business allowed you, with a love for phenomena, with loyalty to experiment, with candour for theory, with genuine enthusiasm you return to pure science, that stamped you, without reference to your ingenuity, inventive power, your gift of observation as an Academician in our eyes. Just because you were not passing through the usual training of the German scientist, the Academy specially counted upon you. Not exactly in the sense that the unusual way in which you soared aloft is a true sign of unusual power, but because thereby, as we commend in many English

physicists, your view remained fresher, your apprehension less confused, your judgment clearer, than if you, like others, had been brought up in the dogmas of the school.

I, who early knew your worth, and have been connected with you in friendship for thirty years, which I consider as the greatest blessing of my life,—I, as speaker for this corporation, could have found nothing more agreeable than to welcome you this day in their name in our midst.

(Address to Mr. Virchow follows.)

CONTRIBUTIONS TO THE THEORY OF LAYING AND TESTING SUBMARINE TELEGRAPH CABLES.*

THE subterranean cables laid in Prussia in the years 1847 to 1852 must be looked upon as the starting point of submarine telegraphy. Experiments had been previously made in insulating underground wires used as conductors with glass tubes, india-rubber, etc., among which those made by Jacobi in St. Petersburg, in the year 1842,† on a rather extended scale, deserve especial mention; they all failed however. In the year 1846, I proposed to the Prussian government the use of gutta-percha as an insulating material, which had shortly before been introduced into Europe. Its property of becoming plastic when heated, combined with its insulating property, made it appear specially applicable for the intended purpose. Yet the experiments simultaneously made with this material both here and in England, were not satisfactory, for the gutta-percha rolled round the wire gave way after a short time at the joints. It was with the help of a covering machine constructed and employed by Halske and myself in the year 1847, by means of which the gutta-percha made plastic by heat, was pressed without a seam around the wire, that the problem of the manufacture of thoroughly insulated submarine and underground cables was solved.

* Monthly Report of the Berlin Academy of Sciences of 17 Dec., 1874.

† Pogg. Ann. Vol. 28, p. 409.

Although the extensive network of underground conductors insulated with compressed gutta-percha which in the following years was so hastily spread over North Germany and Prussia, did not have a very long life, especially since, to save cost, the wires were laid without external protection, and at too slight a depth in the ground, yet it gave an opportunity of collecting experience on the manufacture and maintenance of such insulated conductors, and of studying their physical properties. It was reserved for English enterprise to apply the knowledge and experience gained here in a province from which the competition of cheaper overhead wires is excluded, viz., that of submarine telegraphy.

In the year 1850 Mr. Brett had already laid a single conductor insulated with gutta-percha across the Channel from Dover to Calais. As this, as was to be foreseen, was not permanent, he replaced it in 1851 by a conductor insulated with pressed gutta-percha which was surrounded with a sheathing of strong iron wires for protection against external injury, and thus produced the first serviceable submarine cable.

The laying of this cable presented no great difficulties on account of the small depth of water. The experiments which Brett afterwards made of laying a similar cable also in deep parts of the sea failed, however, as the forces occurring in laying deep sea cables were not then rightly understood, and the necessary precaution for their regulation had not been properly found out. The first successful laying of a deep-sea cable between Cagliari and Bona in the year 1857, in which I was requested to take part, gave me the opportunity of studying the mechanical work of cable laying. According to English practice the cable is coiled in a continuous spiral in one or more circular compartments built in the cable ship in such a way that it may run out over a pulley placed in the axis of the ring without kinking or being stopped in any way. If the ship is supposed to be moving continuously and uniformly straight ahead, and letting the cable sink behind it into the sea, then each portion of the cable which on account of its great suspended length may be considered as perfectly flexible, will sink down to the sea bottom with equal and constant velocity. The distance of each portion of the sinking cable from the surface of the water must therefore be proportional to the time which has elapsed since it left the ship. If the velocity of the ship be con-

stant, these times are proportional to the horizontal distance of the ship, *i.e.*, the cable must form a straight line from the ship to the sea bottom. This straight line sinks parallel with itself to the bottom. The ship must at the expiration of the unit of time be exactly at the point, where the line of the sinking cable cuts the surface of the water. Therefore if each portion of the suspended cable, sinks to the bottom with a velocity v due to its weight in the water, and if the velocity of the ship is represented by c , the angle α which the line of the cable makes with the horizon is determined by the equation--

$$\tan \alpha = \frac{v}{c} \quad . \quad . \quad . \quad (1)$$

if it is assumed that with steady motion of a portion of the cable falling parallel to itself into the water the space is proportional to the force. The weight w of a unit length of the cable in water may be resolved into two components of which the one $w \cos \alpha$ draws the cable through the water to the bottom perpendicularly to its direction, whilst the other $w \sin \alpha$ exerts a pull in the direction of the axis of the cable, and consequently tends to draw down the straight cable along the oblique plane formed by the water on which it rests. The total amount of this last force is $w.l \sin \alpha$, l represents the length of the suspended cable, or as $l \sin \alpha = h$, *i.e.*, is equal to the depth of water, the total pull P is $= w h$, or equal to the weight of the cable hanging from the stationary ship perpendicularly to the sea bottom. If the cable is not held back in the ship by friction, this pulling force p is only opposed by the friction which the water presents to the sliding down of the cable in the direction of its axis. The amount of the latter depends on the nature of the surface and the diameter of the cable. With heavy cables sheathed with iron, it is so small in proportion to the specific gravity of the cable, that by far the greatest proportion of the pull P or $w h$ has to be balanced by friction on board-ship, if it is desired to prevent the cable uselessly sliding down with great velocity into the depth of the sea.

In order to be able to determine properly the amount of the frictional resistance necessary to be applied at each moment on the ship, it is requisite to know the depth of the sea at each place passed over, and to employ a dynamometer which indicates continuously the amount of strain with which the cable leaves the

ship. As further the horizontal component of this strain checks the ship's progress, the force with which the ship is propelled must be sufficient to overcome this resistance and to drive the ship forward with sufficient velocity. When accordingly the steamer carrying the cable had been fitted with a sufficiently powerful brake, and a dynamometer constructed by me, similar to a chain balance, and when its engine power which was much too small to overcome the great brake used for the heavy cable had been supplemented by another stronger steamer taking the cable steamer in tow, the cable was successfully laid in the considerable depths between the places named.

Messrs. Longridge and Brooks have subsequently* submitted the theory of cable laying to a searching enquiry. No objection can be made to it mathematically, and more particularly it thoroughly treats of the case of a cable lying sloping in the water, and of the curve it takes during its submersion, when it is laid with strain on the sea bottom. Regarded physically, however, the work and the deductions made from it leave much room for objection, since one of the fundamental principles assumed, which has an important effect on the results obtained, is incorrect. The work also fails in a clear knowledge of the essential factors and in lucid development of the results given.

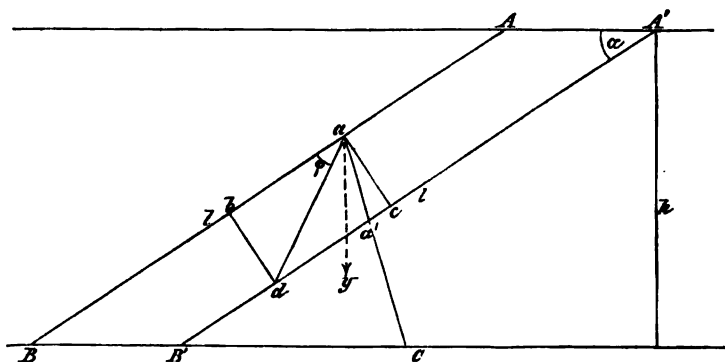
The forces which act on the sinking cable are gravity and the frictional forces opposed to it. Among the latter may be distinguished the sliding friction which opposes the slipping down of the cable in its own direction, and the friction combined with displacement of the mass of water, which occurs with the sinking of the cable in a direction perpendicular to its own. The last is proportional to the square of the velocity of fall, the first to the velocity itself. Longridge and Brooks have assumed both forces to be proportional to the square of the velocity, and hence obtain inaccurate results, especially as regards the determination of the amount of the brake-power which must be employed on the ship.

In the formula previously stated by me $\tan \alpha = \frac{v}{c}$, I have taken the velocity of fall perpendicular to the direction of the cable as

* Proc. of Inst. of C. E., Vol. XVII., 1858. On submerging telegraph cables.

proportional to the velocity ; it will, however, be shown later that for such values of the angle α as generally occur in cable laying this is nearly correct. My assumption that the cable assumes a straight line with uniform speed of the ship is independent of the action of the frictional forces. The cable always assumes a straight line of such inclination, that the component of gravity acting in the direction of the cable is balanced by the sliding friction, that at right angles to this direction by the friction attendant upon the

Fig. 30.



displacement of the water. We thus obtain equilibrium of motion, and therefore uniform motion.

In what follows the letters denote—

- α . The angle the direction of the cable makes with the horizon.
- ϕ . The angle between the direction of the cable, and the direction in which each portion of the cable actually sinks to the bottom.
- c . The speed of the ship.
- u . The constant velocity with which the cable sinks vertically in the water.
- v . The velocity of fall, when the direction of the cable is horizontal.
- w . The weight of a unit length of the cable in water.
- h . The depth of the sea.
- l . The length of the cable suspended in a straight line in the water.

p. The friction and brake-power with which the cable is held back on the ship.

s. The surplus of the length of cable paid out over the simultaneous progress of the ship, or what is technically called slack in English.

A' B' is the position which the cable A B assumes after unit of time. A point *a* of the cable arrives after unit of time at the point *d*. The motion *a d* is resolved into the two motions *a b* and *a c*. All the forces must therefore balance each other in both directions in order that the existing velocity may remain unaltered. The coefficient of sliding friction may be provisionally represented by *r*, that of the friction of the displacement by *q*. The condition of equilibrium of the forces in the two directions *a b* and *a c* gives the formulæ—

$$l \cdot w \sin a - r \cdot l \cdot \overline{a b} - p = 0 \quad . \quad . \quad . \quad (a)$$

$$l \cdot w \cos a - q \cdot l \cdot \overline{a c} = 0 \quad . \quad . \quad . \quad . \quad (b)$$

The remaining quantities are expressed by the relations—

$$\begin{aligned} h &= l \sin a, \\ \overline{a c} &= c \cdot \sin a, \\ \tan \phi &= -\frac{\overline{a c}}{\overline{a b}} \quad . \\ s &= \frac{\overline{a d'}}{c} \quad . \end{aligned}$$

The last relation depends on the fact that the direction in which the point *a* must move when the cable is laid without slack must bisect the angle *cag*, so that B C = B A. For the coefficients *r* and *q* the above-mentioned velocities *u* and *v* must be introduced, namely, for perpendicular fall of the cable in the water—

$$\text{in the vertical direction } u = \frac{w}{r} \text{ from } u r - w = 0,$$

$$\text{in the horizontal } ,, \quad v = \sqrt{\frac{w}{q}} \text{ from } v^2 q - w = 0.$$

u, *v*, *w* are constants of the cable which can be determined before laying. These are the only cable constants, the knowledge of which is here requisite.

For the angle α which the cable makes with the horizon we obtain from formula (b), and the remaining relations :—

$$\cos \alpha = -\frac{1}{2} \cdot \frac{v^2}{c^2} + \sqrt{1 + \frac{1}{4} \frac{v^4}{c^4}},$$

or when $\frac{v}{c}$ is deduced from it—

$$\frac{v}{c} = \frac{\sin \alpha}{\sqrt{\cos \alpha}} = \tan \alpha \sqrt{\cos \alpha} \quad . \quad . \quad (2)$$

This formula is strictly accurate. In practical cable laying, only small values of α generally occur, for which formula (1) gives sufficiently exact results, for with small values of α the expression $\sqrt{\cos \alpha}$ is nearly equal to 1. In the following, however, we will first deduce the consequences that are to be derived from the formulæ (a and b) and the other strictly correct relations, and the formulæ obtained will be simplified later on by the introduction for smaller values of $\frac{v}{c}$ of the approximately correct formula

$$\tan \alpha = \frac{v}{c}.$$

First of all we obtain for the brake-power p with a given slack—

$$p = w h - \frac{c}{u} w h \left(\tan \frac{\alpha}{2} + \frac{s}{\sin \alpha} \right) \quad . \quad . \quad (3)$$

and by putting $s = 0$, for the brake-power P without slack—

$$P = w h - \frac{c}{u} w h \tan \frac{\alpha}{2}.$$

In equation (3) the first term $w h$ has usually greatly preponderating value, so that the brake-power is essentially equal to the weight of the cable, if it be supposed to hang perpendicularly from the ship; this value is at the same time the upper limit for the brake-power, which is nearly reached if the ship lays the cable at a high speed without slack.

From this upper limit two terms are subtracted, which we will represent by P' and S , namely—

$$P' = \frac{c}{u} w h \tan \frac{\alpha}{2}, \quad S = \frac{c}{u} w h \frac{s}{\sin \alpha}.$$

These have very simple meanings ; they are—

$$P' = r l \overline{dc} \qquad S = r l \overline{dd'}$$

\overline{dc} is however the extent to which the cable slips down when it is laid without slack ; $\overline{dd'}$ is the length which is added to \overline{dc} , when it is laid with slack ; P is therefore the amount of the sliding friction in the first case, S that amount which is added in the second case.

P' is at the same time, since $P = w h - P'$, the amount by which when laying without slack the brake-power is less than the weight $w h$; it can be deduced in different ways, that it is almost independent of the speed of the ship, except for very small values of the latter, and is moreover proportional to the depth h .

The quantity S is likewise proportional to the depth, but besides, at least with medium and great speeds, proportional to the square of the ship's speed.

In order to show the dependence of the quantities P' and S upon the others, and especially on the ship's speed, Dr. Frölich, whom I have to thank for his kind assistance in these calculations, has given in the following table a summary of the values P , P' c and s , for all the usual speeds of the ship when the depth is 2,000 fathoms, and s equal to 10 per cent. for the heavy Atlantic cable which Longridge and Brooks refer to, with which—

$$w = 0.3208 \text{ (English pounds).}$$

$$u = 24.201, \quad v = 3.082 \text{ (English feet per second).}$$

TABLE I.

$c =$	2'	4'	6'	8'	10'	12'	15'
$P =$	3617.1	3607.1	3605.0	3604.7	3604.8	3604.8	3604.4
$P' =$	232.5	242.5	244.6	244.8	244.8	244.8	245.2
$S =$	34.2	95.6	198.4	342.9	529.0	756.3	1173.3
$wh =$	3849.6						

In order to show the important differences of the brake weight for a determined value of s , we give below a table of the values of this quantity (p) for the depths of the Atlantic cable, when the slack $s = 10$ per cent.

TABLE II.

	$c = 2'$	$4'$	$6'$	$8'$	$10'$	$12'$	$15'$	$wh =$
$h = 500$ fathoms	$p = 899.7$	877.9	851.7	815.5	769.0	695.9	607.8	962.4
$= 1000$ „	$= 1799.3$	1755.8	1708.3	1630.9	1537.9	1391.7	1215.5	1924.8
$= 2000$ „	$= 3598.6$	3511.6	3406.6	3261.8	3075.8	2783.4	2431.0	3849.6
$= 3000$ „	$= 5397.9$	5367.4	5109.9	4892.7	4613.7	4175.1	3646.5	5774.4

For the angle ϕ , which is the direction of the actual motion of the cable, we have the formula—

$$\tan \phi = \frac{\frac{c}{u}}{1 - \frac{p}{wh}} = \frac{c}{u} \frac{wh}{p'}, \text{ if } p' = wh - p. \quad (4)$$

and finally for s the slack of the cable—

$$s = \frac{p'}{wh} \cdot \frac{u}{c} \sin \alpha - 2 \sin^2 \frac{\alpha}{2}. \quad (5)$$

or
$$s = \sin \alpha \cdot \cot \phi - 2 \sin^2 \frac{\alpha}{2}.$$

For comparison with the above formulæ 2 to 5, we give the following with our letters, taken from the paper of Longridge and Brooks.

Formula 2 remains the same, whilst in place of 3, 4, and 5 we obtain—

instead of (3)
$$p = wh - wh \frac{c^3}{u^3} \frac{(1 + s - \cos \alpha)^3}{\sin \alpha},$$

instead of (4)
$$\tan \phi = \frac{c}{u} \sqrt{\frac{\sin \alpha}{1 - \frac{p}{wh}}} = \frac{c}{u} \sqrt{\frac{wh}{p'}} \sin \alpha,$$

instead of (5)
$$s = \frac{u}{c} \sqrt{\frac{p'}{wh}} \sin \alpha - 2 \sin^2 \frac{\alpha}{2}.$$

The difference between these formulæ and ours lies in the use of the law of the square for the sliding friction; by using them too high a brake-weight is obtained when laying with a known ship's speed and depth, and a determined slack, and, further, too great a slack s , when depth, speed of ship, and brake-weight are given, and s is reckoned from the values of these quantities.

We now introduce the approximation obtained in formula 1, using it in place of formula 2, and by its means eliminate the

angle α , which in practice can hardly be determined from all the other formulæ.

In the first place, let us compare in the following table the values obtained by both formulæ for the cable above referred to, with the given ship's speed :—

TABLE III.

	$c = 2'$		$4'$		$6'$		$8'$		$10'$		$12'$		$15'$	
by (2)	$\alpha = 68^\circ$	$35'$	$41'$	$44'$	28°	$45'$	21°	$47'$	17°	$30'$	14°	$37'$	11°	$44'$
by (1)	$\alpha = 57^\circ$	$1'$	$37'$	$37'$	27°	$21'$	21°	$4'$	17°	$8'$	14°	$24'$	11°	$37'$

From this comparison it follows, that for practical purposes with a ship's speed of more than 8 feet per second, or of about five nautical miles an hour, formula (1) may be considered as correct.

By putting $\tan. \alpha = \frac{v}{c}$ in what follows, and setting aside magnitudes of the order $\left(\frac{v}{c}\right)^2$ we obtain the approximate formulæ—

$$p = w h \left\{ 1 - \frac{1}{2} \frac{v}{u} - \frac{c^2}{u v} s \right\} \quad (3')$$

$$P = w h \left\{ 1 - \frac{1}{2} \frac{v}{u} \right\}, \text{ and} \quad (4')$$

$$s = \left(1 - \frac{p}{w h} \right) \frac{u v}{c^2} = \frac{1}{2} \frac{v^3}{c^2}. \quad (5')$$

Formula (5') shows that the slack s is inversely proportional to the square of the ship's speed ; further, the first important factor in the expression for s is proportional to the difference $w h - p$, i.e., to the difference of the weight of the perpendicularly hanging cable and the brake-power. In laying a cable the slack s may vary from three causes : alteration in the depth h , in the brake-power p , and again in the ship's speed c . If s is differentiated according to these three quantities, we obtain, dividing by s , the percentage alteration s with regard to the same, namely—

$$\frac{\delta s}{s} = \frac{\delta h}{h} \frac{\frac{p}{w h} \frac{u v}{c^2}}{s},$$

$$\frac{\delta s}{s} = - \frac{\delta p}{p} \frac{\frac{p}{w h} \frac{u v}{c^2}}{s},$$

$$\frac{\delta s}{s} = \frac{\delta c}{c} \left(-2 \frac{p}{wh} \right) \frac{uv + v^2}{s \left(\frac{c^2}{c^2} + \frac{v^2}{c^2} \right)}.$$

If for instance $h = 2000$ fathoms, $p = 3261.8$ pounds, $c = 8$ feet, then $s = 0.10 = 10$ per cent.; but in this case—

$$\frac{p}{wh} \frac{uv}{c^2} = 9.9 \text{ and } -2 \left(1 - \frac{p}{wh} \right) \frac{uv + v^2}{c^2} = -2.1.$$

If now for instance, h , p and c are each increased by 10 per cent. of their own value, then s varies in the first case about + 99 per cent., in the second about - 99 per cent., and in the third about - 21 per cent. of its own value; instead, therefore, of having 10 per cent. slack we have 19.9, 0.1 and 7.9 per cent. respectively, and so it is seen that when p , h or c change the slack increases at a much higher rate than these magnitudes themselves, but that the alterations of the slack through alterations of depth and of brake power, are much greater than for variations of the ship's speed.

An important consideration follows from formula 4', namely that the brake power P when laying without slack, unless the speed of the ship be very small only depends on the depth and is proportional to it; Table I. also shows this. But it follows that conversely the depth can be determined from the brake power P . The following tables show how exactly this can be done with various speeds from 4 feet upwards. In Table IV. (p. 248) the brake power is calculated from the strictly correct formula—

$$P = wh \left(1 - \frac{c}{u} \tan. \frac{a}{2} \right).$$

Table V. (p. 248) gives the depths calculated according to the approximately correct formula from these values—

$$h = \frac{P}{w} \frac{1}{1 - \frac{1}{2} \frac{v}{u}}$$

i.e., P is taken as experimentally measured, and the cable constants u , v , w as known, and thence the depth h is determined.

TABLE IV.

h	$c =$	4'	6'	8'	10'	12'	15'
= 500 fathoms.	P =	901.8	901.3	901.2	901.2	901.2	901.1
= 1000	=	1803.6	2802.5	1802.4	1802.4	1802.4	1802.2
= 2000	=	3607.1	3606.0	3604.8	3604.8	3604.8	3604.4
= 3000	=	5410.7	5407.5	5407.1	5407.2	5407.2	5406.6

TABLE V.

h (actual)	h (calculated)					
= 500 fathoms	= 500.3	500.1	500.0	500.0	500.0	500.0
= 1000	= 1000.7	1000.1	1000.1	1000.1	1000.1	1000.0
= 2000	= 2001.5	2000.3	2000.1	2000.0	2000.1	1999.9
= 3000	= 3002.4	3000.4	3000.2	3000.2	3000.2	2999.9

In general it also follows from the approximate formula, that to lay a cable with a determined amount of slack, exact knowledge of the constants of the cables, as well as the depth and the ship's speed are needed. The cable constants we may consider as well determined before the laying; the measurements however of the depth and of the ship's speed can only be imperfectly performed during the laying. The question now is whether there is any means of avoiding or removing these difficulties.

The primary question is, whether the laying can not be effected without brake power. In this case we should have—

$$s = \frac{1}{2} \frac{v(2u-v)}{c^2}$$

i.e., the slack only dependent on the speed of the ship and not on the depth.

To lay without brake power and without slack is according to this formula only possible when $2u - v = 0$; therefore $v = 2u$, *i.e.*, if the cable should have a great sliding friction: in this case however it would not be possible to lay with slack.

If we now suppose that it is desired to lay with 10 per cent. of slack and without brake power, then with the heavy Atlantic cable above referred to, the ship's speed must be 26.4 feet, with the light cable referred to by Longridge ($v = 1.404$, $u = 11.024$, $w = 0.06578$) it must be 12 feet, moreover since here—

$$c = \sqrt{\frac{v(2u-v)}{2s}}$$

by reducing the specific gravity or especially by increasing the sliding friction, a cable might be constructed, which could be laid without brake power, and the regulation of the slack might take place solely by alteration of the ship's speed.

If for any reason it is not feasible to alter the construction of the cable, use might be made of a practical method proposed by my brother, Dr. C. W. Siemens, namely to determine by experiment what brake power must be employed to obtain with the existing conditions the desired amount of slack. It consists in loading the brake, the speed of the ship being constant, until no change in the speed of paying out the cable takes place with a further loading of the brake. The load has then been found for laying without slack for the existing speed of the ship, and the load on the brake can now be easily regulated so that the desired slack may be reached. With a ship in considerable motion and irregularity in the speed of paying out due to it, and with a very irregular sea bottom, this method would also be ineffective.

A previously determined slack can only be obtained with precision, when simultaneously with the cable a rope or wire is allowed to run out, the coefficient u and v of which are nearly the same as those of the cable. If this dummy cable is then held back by a brake power at least so great that it is laid on the bottom of the sea without slack and therefore under strain then a counter applied to it will constitute a measure, unaffected by sea currents, of the exact speed of the ship over the sea bottom, and it is then only necessary to load the cable brake so heavily that the velocity of the cable as it runs out is always in the desired proportion to that of the dummy cable. The increased cost thus incurred would be quite compensated for, because the wire laid without slack does not measure the horizontal speed of the ship, but the length of the sea bottom passed over by it and therefore already takes account in its length of the amount of cable required to follow the inequalities of the sea bottom without strain on the cable. In order to diminish the risk of the occurrence of such a strain on an uneven sea bed, and the formation of long catenaries of the cable, it is customary to lay at least from 10 to 15 per cent. of slack. By the saving in the length of cable paid out the cost of the dummy cable would be fully recouped.

A submarine cable or an underground conductor can only be

guaranteed to have a long life, when the insulation is perfect, *i.e.* when the resistance of the insulating covering is equal to that found by calculation from data of the specific resistance of the insulating materials employed. If there is a decrease of this insulating resistance, it must be assumed that a break in the insulating coating has taken place in one or more places, which allows the water to get to the conductor. This may occur during the manufacture ; it however often shows itself first during laying, or sooner or later afterwards. Therefore both during the construction as well as during and after the laying of a cable, continuous tests of its physical properties should be carried out. If a fault is found to exist, it is of the greatest importance to determine its position, *i.e.* its distance from the ends, with the greatest possible precision. During the laying of cables it is also of importance that this determination should be carried out as quickly as possible, so that whilst the fault is still in the neighbourhood of the ship, the last laid portion of the cable with the fault in it may be picked up. I have already in the year 1850 * given the theoretical basis for the determination of such faults. It consists in obtaining two equations by two measurements of the current and resistance with the help of which the unknown resistance of the fault may be eliminated, *i.e.* the resistance which is offered by the fault to the passage of the current to earth, and then the ratio of the distance of the fault from the ends of the conductor can be determined. The measurement of the current can either be made simultaneously from both ends of the insulated conductor when the further end can be either insulated or put to earth or they can be made from the one end, whilst the further end is insulated for the one measurement and put to earth for the other. As measurements of current are less exact and more difficult than those of resistance, as soon as I had produced a fixed † reproducible unit of resistance, and had used this as a basis on which to construct accurate scales of resistance arranged similarly to a set of weights I converted the formulæ for the position of faults based on current measurements into equivalent formulæ based on resistance measurements.‡

* Pogg. Ann. Vol. LXXIX., p. 192.

† Pogg. Ann. Vol. XC., p. 1 ; Vol. XCH., p. 91 ; Vol. CXX., p. 512.

‡ Outline of the principles and practice involved in dealing with the electric conditions of submarine electric telegraphs, by Werner and C. William Siemens, July, 1850.

If $a b = l$ is the insulated conducting wire, the length and resistance of which are known, if x and y are the distances of the fault from a and b , z the resistance of the fault, the following are the equations which I brought out for determining the distance x of the fault from the end a —

(1.) $\frac{x}{y} = \frac{w}{w'}$ when both ends are in the same room, and w and w' represent the resistances of the branches of the bridge, when no current passes through the galvanometer.

(2.) $\frac{x}{y} = \sqrt{\frac{a(c-b)}{b(c-a)}}$ when a and b are the resistances measured from the two ends, whilst each time the distant end is to earth.

(3.) $x = \frac{a_i - b_i + l}{2}$ when a_i and b_i are the resistances measured from both ends, whilst the distant end was insulated, and l represents the resistance of the faultless conductor.

(4.) $x = l - b + \sqrt{(b_i - b)(l - b)}$ when the letters, l , b_i and b , having the above meaning, measurements were only made from one end of the conductor for the determination of the fault.

As in the first case the variable magnitude of the resistance of the fault, as well as the polarization which occurs in a most disturbing way at the place of the fault do not come into consideration, both determining measurements are made at the same moment, this method where practicable gives particularly accurate determinations of the position of the fault. But the case is quite otherwise with those measurements, in which the ends of the wire are far removed from one another, as in a submerged submarine cable. The tiny openings, often hardly discernible with the eye through which the water passes to the conducting wire, present an extra-

ordinarily variable resistance to the passage of the current. The polarization also which occurs at the place of the fault is often very considerable and variable. The measurements which can be obtained by the application of the above formula are hence only seldom satisfactory and then only when the fault is great, *i.e.* has a low resistance.

Lately two methods have been made known by Messrs. Clark and Jenkin for the determination of the position of a fault in submerged cables, which in great part overcome the uncertainty, inherent to the determination of the fault according to my old method on account of the variation in physical properties at the point of the fault. Mr. Clark insulates one end of the conductor and inserts a battery and a known resistance between the other end and the earth. By means of exactly accordant electrometers the difference of potential of the pole of the battery connected to the resistance and of the cable end, and simultaneously the potential of the other insulated end of the conductor are measured. This last gives the potential of the conductor at the fault and we then get—

$$P - P' : w = P' - p : x,$$

where w represents the inserted resistance, P and P' the measured potentials of its ends, p the potential at the fault measured from the other end of the conductor, and x the resistance of the conductor from the station where the battery is inserted up to the fault, whence follows—

$$x = \frac{w(P' - p)}{P - P'}.$$

As it is supposed that the measurements of P , P' and p are made simultaneously, and either in absolute measure or with instruments agreeing exactly, the variation of the resistance of the place of the fault is actually without influence on the result. Thus the prejudicial influence of the polarization of the fault is eliminated as it has only the effect of increasing the potential of the fault, and therefore acts like an increase of the resistance of the fault. The difficulties of practically carrying out three simultaneous measurements at different places are however very great, and besides electrometer measurements can hardly be made with a sufficient

degree of exactness even with the greatest care on the part of the observer.

The method published by Mr. Jenkin is based on the simultaneous measurement of the current passing through the fault and of the potential at both ends of the conductor. For this purpose a battery and galvanometer are inserted between one end of the conductor and the earth, whilst the other end of the conductor is insulated. Besides both ends of the conductor are connected to electrometers. In the formula of Professor Jenkin—

$$x = - \frac{2}{\sqrt{\frac{k}{i}}} \ln \frac{P + \sqrt{\frac{k}{i}} J - P' e^{\log. e \sqrt{\frac{k}{i}}} }{-P + \sqrt{\frac{k}{i}} J + P' e^{-\log. e \sqrt{\frac{k}{i}}}}$$

in which x is the position of the fault, k the resistance of a unit length of the conductor, i the insulation resistance of a unit length of the cable, J the current through the galvanometer measured in absolute units, and P and P' represent the potentials measured in absolute units at the beginning and end of the conductor, the loss of current through the insulated covering of the conductor is brought into the calculation. As in the case of small faults, the determination of which always causes the greatest difficulty, imperfect insulation is of considerable importance, Jenkin's formula for the determination of faults would be of great value if it did not necessitate simultaneous measurements of the strength of the current and of two potentials in absolute measure and in different places, with an accuracy as regards reliability of result which make it but little applicable for practical use.

It follows from what has been said that a method of determining faults can only give reliable results when the irregular changeable resistance and the variable polarization of the fault are rendered inoperative. For faults with high resistance in long conductors it is necessary to take into consideration or to eliminate the insulation current, *i.e.*, the current of electrification passing through the whole length of the cable up to the fault. The method must further be capable of quick and easy application.

tion can be easily applied, which compensates the influence on the result of the measurement with sufficient exactness for practical purposes.

The carrying out of the potential measurements with sufficient exactness is easy if each end station possesses a sensitive mirror galvanometer, which can be regulated to any degree of sensitiveness by means of a shunt, a resistance of some millions of units, and the means of being able to set up a battery of determined E.M.F. If Daniell cells, with zinc sulphate solution, are used for this battery, and care be taken that the zinc electrode consists of uniform material, and is well amalgamated, and that the liquids be exactly similar in composition, then the same number of such cells will have the same E.M.F. if their temperature be constant. If the latter be the case, and therefore any increase or decrease of the E.M.F. due to thermoelectric currents in consequence of the contact of metals and liquids at different temperatures be prevented, then the E.M.F. of such cells is independent of their temperature. It is now easy to impart equal sensitiveness to both galvanometers by joining up each of them, together with the high resistance belonging to it, and a battery of a predetermined number of cells in circuit, and so regulating the shunt of the galvanometer that its needle gives a deflection arranged beforehand by both stations. Inequalities in the resistance of the battery and of the galvanometer can be neglected, if, as is assumed, the resistances inserted are very great. If the galvanometers, brought to an equal degree of sensitiveness, together with the high resistances, are inserted between the ends of the cable and earth, their deflection gives the potential of the places of contact measured in similar units. A measurable alteration of the potential will not be brought about by this shunt, if the resistance of the batteries and of the whole cable is very small in comparison with it.

The carrying out of the measurements requisite for determining faults by this method simply consists in station A inserting a battery between the end of the cable and earth. When the charge and polarization at the fault have become constant, A and B read the deflections of their galvanometers, and station A thereupon breaks the contact of the cable end with the free pole of the battery. Station B observes this by the decrease in the deflection

of his galvanometer. He then communicates to station A by conventional signals the amount of deflection observed, and then brings the similar free pole of his battery permanently into contact with his cable end. Station A then, by a pre-arranged signal, gives the information whether his galvanometer is more or less deflected than the deflection observed at B. B now increases or diminishes the E.M.F. of his battery, until the signal comes from A that the deflections are equal. As a means of control A and B then alternately connect their batteries with the ends of the cable, and in this way correct the E.M.F. of their batteries, until each produces the same deflection at the other end of the cable. The alteration of the E.M.F. of the batteries can either be brought about by increasing or diminishing the number of cells or by the use of shunts.

As is easily seen, any errors due to the conductivity of the insulator are fully eliminated in these methods of determining faults, if the damaged place lies at or near to the middle of the conductor. If the position of the fault is far from the middle, this is, however, not absolutely, but only partially, the case.

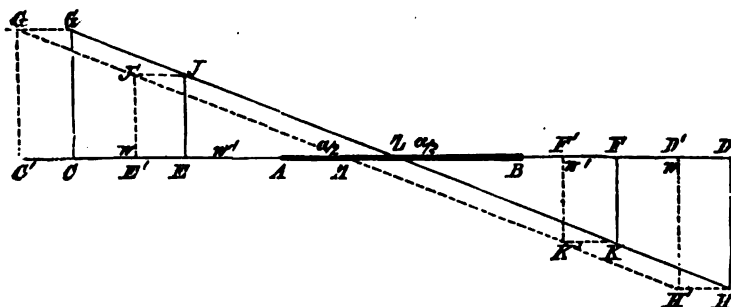
Instead of removing, as in the above method, the injurious influence of the variation of the physical properties of the fault by making the measurements determined on almost simultaneously at both ends of the conductor, so that the fault may be considered as constant for both, this may also be attained by making the electric potential of the fault equal to 0.

If to one end of an insulated cylindrical conductor is joined the positive pole, and to the other end the negative pole of batteries, the free poles of which are to earth, the potential curve cuts the cable in the middle, if the conductor is homogeneous and symmetrically insulated, and if the batteries have equal E.M.F. By putting in and taking out resistances between the batteries and the respective cable ends, this neutral place in the cable can be shifted at pleasure. If it is so shifted that it corresponds with the position of the fault, no current passes through the fault, which therefore is altogether without influence on the strength of the current at the ends of the cable and the form of the potential curve.

If in the following diagram of potentials A B represent the cable, C E and D F equal resistances, E A and B F equal but

variable resistances, further $G J Z K H$ the fall of potential of the cable without fault, the potential difference $G C - J E$ will be increased, and, on the contrary, the potential difference $D H - F K$ diminished if there is a fault at M . If station A now increases its variable resistance $E A$, and station B at the same time diminishes its variable resistance $B F$, until at both stations the previously measured difference of potential $G C - J E = D H - F K$ is again produced, the dotted line $G' M H'$ represents the now existing fall of potential; and then the resistance inserted at A and that withdrawn at B measure the displacement of the neutral point in the cable, therefore also the distance of the fault from

Fig. 32.



the middle. If the measurement is rightly determined, the resistance inserted at the one station will equal that withdrawn at the other. The potential difference $C G - E J$ corresponding to $D H - F K$ can be measured as above, by the discharge of a condenser, the coatings of which are connected with C and E and D and F respectively, or by the deflection of a delicate galvanometer, the ends of the coils of which are connected by very high resistances with C and E and D and F respectively.

Besides the above-mentioned faults of insulation, in which it is assumed that the conductor itself is not injured and stretches continuously from one station to the other, there are faults of other kinds. The conductor may be broken inside the insulating coating, and the metallic connection thus interrupted, or the whole cable may be fractured, in which case the ends of the conductor almost always come into conductive connection with the water.

In the first case the distance from the position of the fracture can be easily determined, by measuring the capacity of the Leyden jar, which is formed by one of the two pieces of the conductor, and comparison with the capacity of unit length of the conductor. This is effected either by reading the deflection of a mirror galvanometer on charging or discharging respectively, or by the proposal of de Sauty and Varley, by passing the charge of the cable to be measured and that of the condenser serving as a comparison simultaneously through the same circuit, and so regulating the branch of a Wheatstone bridge, or of a differential galvanometer, with the help of frequently repeated charges that the galvanometer is not deflected. The proportion of the bridge branches then gives the proportion of the charges.

These simple methods which are very suitable for short cables are not sufficiently exact when the cables are very long. In the first place, too much time elapses before the charge of the cable is complete, and, secondly, the galvanometer must be made too unsensitive in order to be still able to measure the great quantity of electricity collected in a long cable with the necessary exactness. This also holds good in the use of de Sauty's differential measurement, for with too sensitive galvanometers the charging current of the condenser, which is much stronger at the beginning, throws the needle of the galvanometer over in its direction, whilst the slowly moving current which charges the cable moves it later in the opposite direction.

The deficiencies of the means at present known can be removed by making use of the discharge deflection of a condenser of known capacity charged by a constant battery; then using the same condenser as a unit jar for repeated partial discharges of the cable, and finally also measuring the n th discharge of this unit jar. Let k be the capacity of the comparison condenser if the unit cable length is taken as the unit of capacity; also x the capacity of the whole cable for the length x . Further, let P be the potential to which the cable and comparison condenser are charged, $P_1, P_2, P_3, \dots P_n$ the potentials of the cable, as well as of the condenser connected to it after the first, second, &c., n th discharge of the latter. Lastly, let a and a_n be the discharge deflections of the condenser with the first or n th discharge, the following proportions then hold—

$$P : P_1 = x + k : x$$

$$P_1 : P_2 = x + k : x$$

$$\dots\dots\dots$$

$$P_{n-1} : P_n = x + k : x$$

consequently

$$P : P_n = (x + k)^n : x^n$$

or

$$\sqrt[n]{P} : \sqrt[n]{P_n} = x + k : x$$

$$\frac{\sqrt[n]{P} - \sqrt[n]{P_n}}{\sqrt[n]{P_n}} = \frac{k}{x}$$

$$x = \frac{k \cdot \sqrt[n]{P_n}}{\sqrt[n]{P} - \sqrt[n]{P_n}}$$

or if a and a_n represent the deflections of the galvanometer corresponding to the charges of the comparison condenser produced by P and P_n ,

$$x = k \frac{\sqrt[n]{a_n}}{\sqrt[n]{a} - \sqrt[n]{a_n}}$$

The determination of the point of fracture of a cable is much more difficult when, as is usually the case, the end of the fractured conductor is in conductive connection with the water.

It generally happens that the fracture occurs so that the conductor and insulating covering do not break at the same point, consequently either a piece of the wire reaches into the water, or it is in conductive connection with the surrounding water only through the narrow tube incompletely filled with water. In the first case resistance measurements carefully made from land give sufficiently accurate results. Besides the variable polarization of the place of the fault, there is also another very troublesome circumstance that almost uninterrupted so-called earth-currents pass through the conductors in greater or less quantity. Also when auroræ boreales do not make their appearance at night, currents which are to be ascribed to terrestrial and cosmical causes are often produced in cables, both ends of which are in conductive connection with the water, these may have an E. M. F. equal to that of six to eight Daniell cells. I have been able to compensate the disadvantageous influence of these earth-currents on the

and

$$y \, dx = dQ,$$

$$\int_0^l y \, dx = Q.$$

The quantity of electricity $x \cdot dx = dQ$ when both ends C and D of the conductor are in connection with the earth, and the electromotive force P which produces the charge is removed at A, will flow towards both sides. Let dQ_1 represent the part of dQ which passes to earth through A and C, whilst dQ_2 represents the part passing through B and D to earth. These quantities must vary inversely as the resistances through which they flow. We get therefore—

$$dQ_1 : dQ_2 = l + z : x + w,$$

or in

$$dQ_1 + dQ_2 = dQ$$

$$dQ_1 = \frac{y \cdot dx (l + z - x)}{w + l + z}.$$

As now further

$$y : P = l - x + z : l + z,$$

therefore

$$y = P \cdot \frac{l + z - x}{l + z}$$

and so

$$dQ_1 = P \cdot \frac{(l + z - x)^2}{(w + l + z)(l + z)} dx,$$

or

$$\begin{aligned} Q &= \frac{P}{(w + l + z)(l + z)} \int_0^l (l + z - x)^2 dx \\ &= \frac{P}{3(w + l + z)(l + z)} \left((l + z)^3 - z^3 \right). \end{aligned}$$

If one places in this equation the value $a = l + z$ found by

making a resistance measurement at the same time as the charge of the cable, there is obtained for the value of the return charge—

$$Q_r = P \cdot \frac{a^3 - z^3}{3(w + a)a}$$

and thence

$$z = \sqrt[3]{a^3 - \frac{3 Q_r (w + a) a}{P}}$$

Or as $P l = 2 Q$ is the amount of the charge of the whole of the insulated faultless cable, consequently P is to be put equal to the amount of the charge q of the unit of insulated cable—

$$z = \sqrt[3]{a^3 - \frac{Q_r}{q}, 3(w + a) a}.$$

As $l = a - z$ is known from the measurement of resistance made simultaneously, the length of the fractured cable l will also be given in this way.

If the surface resistance $z = 0$, consequently also $a = l$, there follows from the above equation for x —

$$l q = 3 Q_r, Q_r = \frac{l \cdot q}{3} = \frac{2 Q}{3}$$

which means : that if a charged cable, the further end of which is in connection with the earth without resistance, is connected to the earth ; $\frac{2}{3}$ of the charge of the cable goes back to the charging station, whilst $\frac{1}{3}$ goes to the earth at the further end.

Naturally, between the removal of the battery and the insertion of the galvanometer connected to earth, no time must be lost, as otherwise during the insulation of the charged end, an important part of the electricity goes to earth through the other end, and the measured recharge therefore comes out too low. If the charge of connection is, however, so arranged that it is effected instantaneously, as Helmholtz already did in the year 1851, the method gives very conformable and exact results with conductors that are not too long. If the conductors to be experimented upon are very long, the retardation of the current in consequence of the charge introduces a disturbing element. The above formula therefore requires a correction in this case for the retardation of the current, the enunciation of which I have so far failed to discover.

ON THE INFLUENCE OF LIGHT ON THE CONDUCTIVITY OF CRYSTALLINE SELENIUM.*

I have further investigated the property of crystalline selenium first described by Willoughby Smith, and more closely studied by Sale,[†] of conducting electricity better in light than in darkness, and have proved the correctness of the statement. The specific conductivity of selenium rendered crystalline by heating it to 100° or 150° C. is, however, very slight and exceedingly variable, and the increase of its conductivity under the influence of light is very irregular, so that it was impossible to discover a definite dependence of conductivity on illumination. By continuously heating amorphous selenium up to the temperature of 210° C., as well as by cooling the molten selenium to the temperature of 210°, by long continuance at which temperature the selenium passes into a coarse grained state, I succeeded however in producing another modification of crystalline selenium having considerably higher conductivity which continues permanent, and conducting electricity metallically, so that the conductivity diminishes with increase of temperature. The action of light on this modification of crystalline selenium is also much greater, and apparently quite constant. By placing two flat spirals of wire about 1^{mm} apart, between two plates of mica, and running in molten coarse grained selenium, I have succeeded in producing a very sensitive photometer. Dark heat rays have no direct influence on its conductivity, and heating the selenium diminishes it. Diffused daylight already doubles its conductivity, and direct sunlight raises it in some cases more than ten times. The increase of the conductivity of coarse grained selenium when exposed to light is particularly rapid. Its decrease on exclusion of light is apparently instantaneous, though some time elapses before the condition corresponding to darkness is again fully re-established. The increase of conductivity is not proportional to the intensity of the light, but is a function of it which increases nearly in the ratio of the square root of the intensity of the light.

* Monthly Report of the Berlin Academy of Sciences of 13 May, 1875.

† Proc. of Roy. Soc. Vol. XXI., p. 233; Pogg. Ann. Vol. CL., p. 333.

I propose making a more detailed communication to the Academy on this interesting property of selenium after the completion of my experiments, and now only remark that I hope to be able to employ it in the construction of a reliable photometer.

MEASUREMENT OF THE VELOCITY OF TRANSMISSION OF ELECTRICITY IN SUSPENDED WIRES.*

THE continual frosty weather of last winter, and the friendly assistance of the management of the Lower Silesian Railway, and especially of its telegraph inspector, Mr. Wehrhahn, rendered it possible for me to carry out the proposal already made by me in the year 1845 † for the direct measurement of the velocity of propagation of electricity. Unfortunately the coming on of damp weather during the experiments hindered their completion, yet the results already arrived at appear important enough to be brought forward prior to the completion of the work.

The method employed in this instance differs in some essential points from my earlier attempt. In this latter two steel cylinders rotating uniformly and insulated from one another and from the ground, were necessary for making the measurement, and two twin conductors were required, one of which was connected with both cylinders, the other with two insulated points which were placed in close proximity to the periphery of the cylinder. On discharging a Leyden jar between one point and the end of the wire belonging to it, the discharge current must pass through the whole circuit and leave a mark of the spark on the surface of each of the two steel cylinders. The difference between the distances of the marks produced during the rotation of the cylinders, from those produced in the same way when the cylinders were motionless, was the measure of the time the electricity required to pass over one-half the circuit.

Many difficulties stood in the way of carrying out this plan. There was first of all the difficulty of arranging four thoroughly

* Monthly Report of the Berlin Academy of Sciences of 6 Dec., 1875.

† Pogg. Ann. Vol. LXVI., p. 435.

well-insulated conductors of equal length starting from the same point, but the principal difficulty was the mechanical one of setting up steel cylinders perfectly insulated from the earth and from one another, so light and so perfectly balanced that the necessary velocity of 100 to 150 revolutions per second could be given to them. I therefore arranged another method in which only one uninsulated cylinder and only one twin wire was required.

It consists in arranging two Leyden jars or condensers, the inner coating of one of which was connected directly by a short wire, and that of the other by the long conducting circuit, with the point standing nearly opposite to the rotating cylinder connected with the earth. The outer insulated coatings of the jars are in metallic connection. If they are connected with earth the electricity of the interior coatings of both jars becomes free at the same moment, and discharges itself through the point and the rotating cylinder to earth. If the rotation is excessively quick, and the circuit long enough, two marks will be found on the cylinder separated by an interval, the distance between which is the measure of the time which the electricity required for passing through the circuit from the jar to the point.

I also modified the arrangement so that instead of one point I placed two of them opposite to the surface of the cylinder, and connected one point directly with the one jar, the other through the conductor with the other jar. The points were placed as nearly as possible opposite to one another so that the simultaneous marks produced when the cylinders were at rest lay quite close together, and as far as possible in one plane parallel with the axis. A discharge of the jar was first made with the cylinder at rest, and then the discharge serving for the measurement was made with the cylinder rotating. The apparatus itself was the same as I used for the measurement of the velocity of motion of cannon or gun shots, which I have described elsewhere. The steel cylinder is turned from a massive steel cylinder so as to be as light as possible. Its diameter is 40^{mm} and its height 10^{mm}. Its steel axle is provided with a screw into which the teeth of a spur wheel gear. This is uniformly rotated by means of wheel-work driven by a weight. The velocity of rotation of the cylinder may be varied at will between wide limits by means of a regula-

tor already described elsewhere. The spur wheel, provided with 100 teeth, carries a little projection, by means of which after each revolution a light hammer is raised, which strikes a little bell. When the regulator is so set that the strokes of the bell agree exactly with the ticks of a seconds' pendulum, the cylinder rotates exactly 100 times in a second. Opposite to the surface of the cylinder is a small microscope fitted with a cross wire, which serves for reading the angular distance of the marks of the spark.

When at rest the movement of a lever can bring an endless screw with a milled head into gear with the cylinder, by means of which the latter can be slowly revolved until the cross wire of the microscope passes through the middle of the mark of the spark. In this way the millionth of a second can be exactly read, and the 10 millionth estimated.

The conducting point opposite to the surface of the cylinder consists of a thin glass tube into which a very fine platinum wire is fused. After this glass tube has been encased in a metal tube with screw threads, and its end opposite to the cylinder has been carefully ground to the form of a hemisphere, it is screwed as nearly as possible up to the surface of the rotating cylinder.

The glass covering which surrounds the platinum wire up to its extremity serves to prevent sparks from striking across sideways. Very weak sparks leave on a polished steel surface a single bright gleaming point, stronger ones a bundle of sparks, on the middle of which the cross-line must be placed. For the purpose of finding the spark marks more easily, the cylinder before use is covered in the usual way with a soot-film. In this way the very weakest sparks, hardly visible to the naked eye, are surrounded with a distinct annular circle, which makes it possible to bring it easily into the field of view of the microscope. Instead of the Leyden jar I made use as a rule of condensers of tin-covered leaves of mica. These were carefully melted into a mass of rosin, and were thus able to retain the charges employed for a long time without noticeable loss. They were provided with a commutator by means of which they could be simultaneously charged by a Holz machine, while insulated from the point (or the two points, when two of them were used), and then at the last moment before the experiment the coverings previously connected with earth could be connected with the point or the respective points, whilst

the other conductively connected coverings ended in a wire insulated with gutta-percha. The discharge was brought about by a knife in conductive connection with the earth, being driven by means of a powerful hammer through the insulation of the wire, and so a short but as far as possible resistanceless connection was made between the connected coverings and the earth. In this way it was possible to remove altogether from the cylinder the faulty discharge marks, caused by the slow discharge of the condenser, which were at first very troublesome.

With the apparatus so arranged, a series of experiments were first carried out in the laboratory. It was proved that the discharge of a jar in a discharging circuit of slight resistance occurs so quickly that the bundle of marks produced on the rotating cylinder was not very different from that on the cylinder at rest. Individual spark marks which occur almost always irregularly on the surface of the cylinder are evidently to be ascribed to the so-called residual charge of the condenser. The phenomenon is different when the discharge takes place through very great resistances. In this case a continuous series of spark marks is formed on the cylinder, never however a continuous line, which would correspond to an electric current lasting for a measurable time. It must not however be concluded from this, that the total discharge consists also in this case of a series of partial discharges of immeasurably short duration. If, on the contrary, one considers the discharge as consisting of a continuous current of diminishing strength, and the sparks consequently as persistent electric arcs, this appearance of a series of separated spark marks may be explained.

By the rotation of the cylinder the layers of air nearest its surface are carried along, and more perfectly the nearer the air layer is to the surface of the rotating cylinder. If it is now assumed that the commencement of the discharge has broken through the air layer between the point and the cylinder, and being carried round with it, has thereby created a good conducting and incandescent channel between the point and the cylinder, then this channel is carried on by the rotation. If subsequently a more permanent passage of electricity takes place from the point, the channel becomes continually lengthened by this, as it presents in spite of the greater length a less resist-

ance to the electricity than the uninterrupted cold air, which is interpolated between the point and the wall of the cylinder. If this discharging passage has already reached a certain length, its resistance becomes greater than that of the cold air between the point and the cylinder, a new rupture occurs, and with it the formation of a new spark mark and discharge channel.

The discharge of a jar through an india-rubber tube filled with water or through a wet string, gave as it appeared a series of fine spark marks encircling the whole cylinder several times; no loss of time in beginning the discharge was however noticed. As it appeared probable to me from many causes, and especially in consequence of the results obtained by Fizeau and Gounelle that the velocity of electricity must be proportional to the specific conductivity of the material, I repeated this experiment with an india-rubber tube 100ft. long and 20^{mm} bore, which was filled with a solution of sulphate of zinc. To my great surprise no difference could be found in the time between the direct discharge marks and the first partial discharge through the tube of liquid 100ft. long. As a difference of the 5 millionth of a second could be easily observed, it follows that the velocity with which electricity passes through liquids must exceed 800 geographical miles per second. 42

As the conductivity of copper is at least 200 million times greater than that of the sulphate of zinc solution, the velocity of electricity in copper must be at least 160,000,000,000 miles if the specific conductivity were synonymous with the velocity of electricity.

It can hardly be assumed that electrolytic conductors can conduct electricity quicker than metallic conductors of equal conductivity; the contrary is more likely, for it must be assumed, that molecular motion occurs in electrolytic conduction.

The question had now to be decided by means of experiments carried out with longer telegraph conductors, whether to electricity like light is to be ascribed a determined measurable velocity; or whether the retardation values measured by different observers are to be ascribed altogether or in great part to the retardation of the appearance of the current at the distant end of the conductor through the inductive charging of the wire. To this end the experiments should be carried out shortly after one another with

wires of the greatest possible difference in length, and the inductive capacities of these lengths of wire measured each time.

The first experiments were made on the 23rd of February of this year in Köpenick, whither Dr. Frölich who, with his usual care and ability, carried out the following measurements there, as well as later in Sagan, had already gone with the apparatus.

First it was proved by a series of experiments that the insulation of the conductor sufficed, with the prevailing clement frosty weather, to conduct the discharge sparks through the whole telegraph conductor of 5^{mm} thick iron wire leading to Eckner distant 12·68 kilometres and back to the rotating cylinder.

The experiments were made with two points, *i.e.* the one smaller jar was discharged directly through the one point and the second considerably greater jar through the conductor and the other point. Seven discharges were made. The readings taken on the following day gave—

122·8
111·7
125·3
142·7
117·6
121·8
134·3

or as a mean 125·2 millionths of a second.

As the length of the conductor there and back amounted to $2 \times 12·68 = 25·36$ kilometres, this gives a velocity of 202600 kilometres or 27,800 geographical miles per second. It was remarked that the direct discharging sparks of the little jar passing through the one point, always formed a small bundle of spark marks surrounded by a large concentric circle, from inside of which the soot was flung away, whilst by the second point a series of smaller sparks was produced, which were surrounded with no ring or only a very slight one.

Often also a slight point was visible in line with the last point, exactly opposite to the local discharging mark. This was either in consequence of a back or side discharge from the cylinder to the neighbouring point, or more likely an induction taking place

between the part lying next to the cylinder of the out and return conductors fastened on the same poles. In general, the local discharge was much stronger than necessary, which brought this disadvantage with it, that the first point of the line discharge fell frequently in the circle of the local discharge, and was therefore difficult to find out.

Owing to the occurrence of a thaw in which the insulation of telegraph lines is not sufficient for conducting frictional electricity, the further experiments intended were hindered for a long time. When the frost set in again later, the double line passing from Sagan station to Malmitz, and to a signal station lying between Sagan and Malmitz, were placed at our disposal by Mr. Wehrhahn, —Dr. Frölich who repaired to Sagan with the apparatus, succeeded in making two valuable series of observations. They were made partly with two, partly with one point. In these experiments the double point always made its appearance, and Dr. Frölich satisfied himself by a series of test experiments that this double or rather initial point had a local cause, and could not be due to electricity which had to pass through the whole conductor. The line discharges here produced a tolerably long tail of 6 to 8 points, whose distance from one another amounted at first to about 30 and at last 15 to 20 millionths of a second, and after it frequently followed a short line without distinct points. This harmonized very well with the previous explanation of the appearance of discharging points with continuous discharge. The stronger the discharging current the longer is the discharging channel on the periphery of the rotating cylinder and the wider the points must be separated from one another. When the discharge is nearly ended the strength of the current and the evolution of heat is so slight, that no further discharging channel can be formed, the series of points consequently changes into a feeble line.

The double line from Sagan to Malmitz, 11·686 kilometres distant, was first used. The readings of 22 discharges gave—

100·4	88·7	108·7	104·2
102·7	102·6	101·1	104·2
91·2	95·6	108·3	107·3
100·8	97·5	102·0	110·3
100·6	100·5	104·2	
91·4	104·7	102·6	

or an average of 101·4 millionths of a second. As the distance traversed was $2 \times 11\cdot686$ kilometres = 23·372 kilometres, the velocity was 230500 kilometres = 31,060 geographical miles.

The double line from Sagan to the signal station, 3·676 kilometres long, was then inserted and gave with 12 discharges—

39·4	23·0
41·9	25·9
27·8	30·5
27·0	22·1
35·6	28·9
28·4	34·8

or an average of 30·4 millionths of a second. This gives a velocity of 241800 kilometres = 32,590 geographical miles.

A subsequent series of 13 discharges with one point, of which Dr. Frölich places less dependence, because the regulation of the clockwork was less carefully carried out, gave—

87·8	78·2	80·8
76·4	96·3	96·3
84·5	93·1	93·5
93·2	85·5	101·2
		117·9

on the average 91·1 millionths of a second, consequently a velocity of 256600 kilometres or 34,580 geographical miles.

If these measurements do not agree to the extent which is to be expected from the method employed, and which will be gained by a repetition of the experiment under favourable circumstances, yet they show that the motion of electricity in conductors takes place with a determined velocity independent of the length of the conductor, which in iron wires lies between 30,000 and 35,000 miles a second. In consequence of the results obtained with the india-rubber tube, before these experiments were made, I was inclined to the view, that the actual velocity of electricity is immeasurably great, and that the retardations found by Wheatstone, Fizeau, and others were altogether caused by the inductive action of overhead wires.

If that were so, then the Sagan-Malmitz line—which is nearly

three times as long as that from Sagan to the block-house—conductor must have given a retardation nearly 9 times greater, whilst the velocity resulting from the experiments made under like conditions with double points were as 31 : 32·6. Yet putting aside these numbers, which contradict the law of squares of retardations, the retardation is much too great to admit of explanation by delays through induction. The inductive capacity of both conductors was measured by Dr. Frölich with the continuous commutator according to the method used by me for ascertaining the charging law.* The measurement gave

For Sagan Malmitz—

				Microfarads
Galvanometer in the charging circuit	.	.	.	0·181
„ „ discharging „	.	.	.	0·120
				<hr/>
as a mean				0·1505

For Sagan Block-house—

Galvanometer in the charging circuit	.	.	.	0·066
„ „ discharging „	.	.	.	0·061
				<hr/>
as a mean				0·0635

whence results on the average an electrostatic capacity of the overhead 5^{mm} wire of 0·058 microfarads per mile.

As unit of capacity the microfarad has been chosen, which is used in submarine cable work, and is derived from Weber's absolute unit of electric quantity.

For the direct comparison of the measured values of retardation with those which must follow as a consequence of the charging of the wires, the retardation measurements which Dr. Obach made in my laboratory with the help of an artificial cable, *i.e.* a series of 32 condensers of about 20 microfarads, which were connected together through resistances of 550 units each, will serve.

The measurements were made with my exceedingly sensitive electrodynamic relay, free from iron, and a chemical writing telegraph with a double needle.

* Pogg. Ann. Vol. CII. p. 66.

1. 32 divisions of the cable box were inserted. They represented a resistance of 17,600 mercury units = W and a capacity of 639.6 microfarads = C . This gave a retardation of 0.72 second, and therefore 0.0640 second per million of the product of resistance and capacity ($W \times C$).

2. 24 divisions inserted—

$$W = 13200 \text{ mercury units}$$

$$C = 488.9 \text{ microfarads}$$

gave a retardation of 0.45 second

or 0.0715 per million.

3. 16 divisions inserted—

$$W = 8800 \text{ mercury units}$$

$$C = 319.6 \text{ microfarads}$$

gave a retardation of 0.22 second

or 0.078 per million.

These give on the average a retardation for 1 million of 0.0712 second.

The Sagan-Malmitz conductor and back had, according to Dr. Frölich's measurements—

$$\text{a capacity } C = 0.151 \text{ microfarads}$$

$$\text{a resistance } W = 189.0 \text{ mercury units}$$

$$\text{and consequently } W \times C = 28.5$$

hence a retardation of 2.0 millionths of a second could be obtained with the inductive charge, assuming the law of squares, whilst it could only amount to 0.3 millionth of a second with the Sagan Block-house line.

If one also takes into consideration that these times of retardation must have been considerably greater than with the cable measurements, as it takes a longer time for the electric potential of the spark producing points to be great enough for the spark to spring over to the cylinder, then it is evident that the retardation measured for instance on the Sagan Block-house section of 30.4 millionths of a second must have some other cause than the inductive retardation reckoned at 0.3 millionth of a second.

I hope in the course of this winter to find an opportunity not only of repeating the above experiments under better conditions and with better apparatus, but of extending them to a copper conductor, so as to determine by direct measurement the question whether the velocity of electricity depends on the nature of the metallic conductor or not. From the experiments made with the india-rubber tubes filled with the sulphate of zinc solution, the latter appears to me to be likely. Kirchhoff has calculated on the basis of Weber's fundamental law for the motion of electricity, the number 41,000 miles for the velocity of electricity in conductors, and has thus come to the conclusion that this velocity must be alike in all conductors. Our measurements approximate at least more closely to Kirchhoff's value than to that of Wheatstone estimated at 61,900 geographical miles from the lagging behind of the middle spark.

Fizeau and Gounelle have found with the help of their differential method of measurement for galvanic currents in telegraph wires for copper 177,792 kilometres, for iron 101,701 kilometres, being for iron only about half as great a velocity as our measurements have given.

Walker, Mitchell and Gould on American telegraph lines have obtained even still smaller velocities, with electromagnet registering apparatus, the last only 12,851 English miles. No great weight is to be placed on these measurements, as the inertia of electromagnetic instruments is too great and irregular for the measurements of such small intervals of time. The measurements of Fizeau and Gounelle appear of much greater importance. They have not made any allowance for the retarding influence of the charge to which I first drew attention after their experiment was made, and in the description of their experiments the necessary data are wanting to enable us to calculate as a correction the charge of retardation. If however the retardation of the charge due to the comparatively great lengths of their conductor (about 300 kilometres) be a thousand times greater than in my experiments, it does not suffice to explain the difference. I therefore think that the difference in the velocity of electricity in iron and copper found by Fizeau, is not yet to be regarded as confirmed.

ON THE DEPENDENCE OF THE ELECTRIC CONDUCTIVITY OF SELENIUM ON LIGHT AND HEAT.*

SELENIUM, which was discovered by Berzelius in 1817, is, like tellurium, on the boundary between metals and metalloids, and possesses the physical as well as the chemical properties of both classes of bodies.

The physical properties of selenium are specially treated of by Hittorf † in his communication on the allotropy of selenium. He found that it melts at 217°C . ; that it remains fluid when cooled far below its melting point ; that when rapidly cooled still further it solidifies, without giving up its latent heat of fusion, to a glassy amorphous mass of a somewhat greenish appearance, which does not conduct electricity, and has a specific gravity of 4.276. If this amorphous selenium is again heated, it begins to change at 80°C . It has a white metallic appearance, a fine grained crystalline fracture ; its specific gravity increases to 4.796, and thereby disengages so much heat that often a large quantity of it is heated to the melting point. In this crystalline state it conducts electricity like carbon, tellurium, and the electrolytes, whilst its conductivity increases with increasing temperature. Near its melting point the conductivity is very great compared with that at atmospheric temperature. If the melting point is exceeded, its conductivity diminishes considerably with the giving up of its latent heat ; but it still conducts electricity in the melted state.

Owing to the observation made by Superintendent May, of the Valentia Cable Station, that the conductivity of selenium is increased by illumination, an observation published by Willoughby Smith, and afterwards confirmed and more closely inquired into by Lieutenant Sale, the attention of physicists is at present very much directed to selenium.

Sale found that light of all colours increased the conductivity of selenium ; that the dark actinic rays of the spectrum exercised no influence upon it ; and that from this point the light effect

* Monthly Report of the Berlin Academy of Sciences of 17 Feb., 1876.

† Pogg. Ann. Vol. LXXXIV., p. 214.

increased to the red ; that it diminished at the ultra red, and that the effect of the dark heat rays lying beyond is but small.

I have confirmed these results of Sale's in a preliminary communication brought before the Academy in May of last year. I succeeded, by heating amorphous selenium for many hours continuously to a temperature of from 200° to 210° , in producing a modification, which, at the temperature of the atmosphere, conducts twenty to thirty-nine times as well as, and is correspondingly more affected by, light than the selenium made crystalline by heating from 100° to 150° . This modification has the further property of conducting electricity like metals, *i.e.*, so that the conductivity diminishes with increasing temperature. I further found that the light does not affect the whole substance of the selenium, but is essentially a surface action. Led so far, I succeeded, by melting selenium in between the spaces of two flat spirals of wire coiled within each other, in producing a preparation extremely sensitive to light, which I employed for the construction of a selenium photometer. Lastly, I proved that the increase of conductivity of selenium on exposure to light is approximately proportional to the square root of the intensity of the light.*

Professor W. G. Adams has contemporaneously with myself, been investigating the action of light on selenium. He found, contrary to Hittorf, that the conductivity of his pieces of selenium (about the production of which he gives no information) diminished with increasing temperature, thus showing a similar behaviour to what I produced by exposing selenium to a continuous heat of 200° C. He also found that the resistance of selenium measured by a Kirchhoff-Wheatstone bridge was the less the greater the number of cells in the battery employed for the measurement. Adams leaves it undetermined whether the action of light on selenium consists in a change of its surface or whether a polarization current was produced by illumination of the selenium, which opposed the current to be measured, and so increased its conductivity. He seeks to explain in the same way the reduction of the resistance of the selenium when stronger batteries are used. But here he has evidently fallen into an error, for such an oppos-

* Proc. Roy. Soc. Vol. XXIII., p. 535, June, 1875.

ing current produced by light or by the current must have had the opposite effect. Light would have to diminish the conductivity, and by the use of stronger batteries a greater resistance would have to be observed.

I next set to work to discover whether other bodies had the very remarkable property of selenium, of becoming better conductors under the influence of light. These attempts were, however, entirely without result. I thought I had discovered an analogous action in tellurium, but I was soon satisfied that the slight increase of conductivity observed was to be ascribed to the heating of the tellurium by light and heat rays. As I had consequently to assume that it was no question here of a general property of light, but of an abnormal action of the selenium, I resolved to study more closely the relation of this body to heat and to the galvanic current, in the hope of thus obtaining information for explaining the action of light upon it. I first repeated Hittorf's experiment with my instruments, which were better suited for measuring galvanic currents.

As glass, and even porcelain, conduct electricity at high temperatures, I made use of a piece of steatite, which is a perfect insulator even at a red heat, to produce a thick walled crucible, which could contain six grammes of selenium. A thermometer reached through the well-closed steatite cover right into the middle of the hollow of the crucible. The side of the crucible about 10^{mm} thick, was bored through at about half its height, and the two holes closed with an exactly fitting cylinder of gas-coke, which projected both inwards and outwards. After the crucible was filled with melted selenium and then quickly cooled, so that it was filled with amorphous selenium, the outer ends of the gas-coke cylinders were connected with the well-insulated conducting wires of my very sensitive reflecting galvanometer, with a dead-beat bell magnet and a Daniell cell inserted in the circuit, after I had convinced myself that, even when a battery of 100 cells was inserted, no current passed through the amorphous selenium. The crucible so prepared was now quickly immersed in a large vessel of paraffin at a temperature of 280° C., and maintained during the experiment as exactly as possible at this temperature, and the temperature of the selenium in the crucible, as well as the deflections of the mirror, simultaneously and continuously observed and noted. Owing to the high resistance of selenium, in consequence

of which the resistance of the selenium between the carbon points even at high temperatures is always very great in comparison with that of the galvanometer, the deflections of the mirror can, without essential error, be considered as proportional to the conductivity of the selenium.

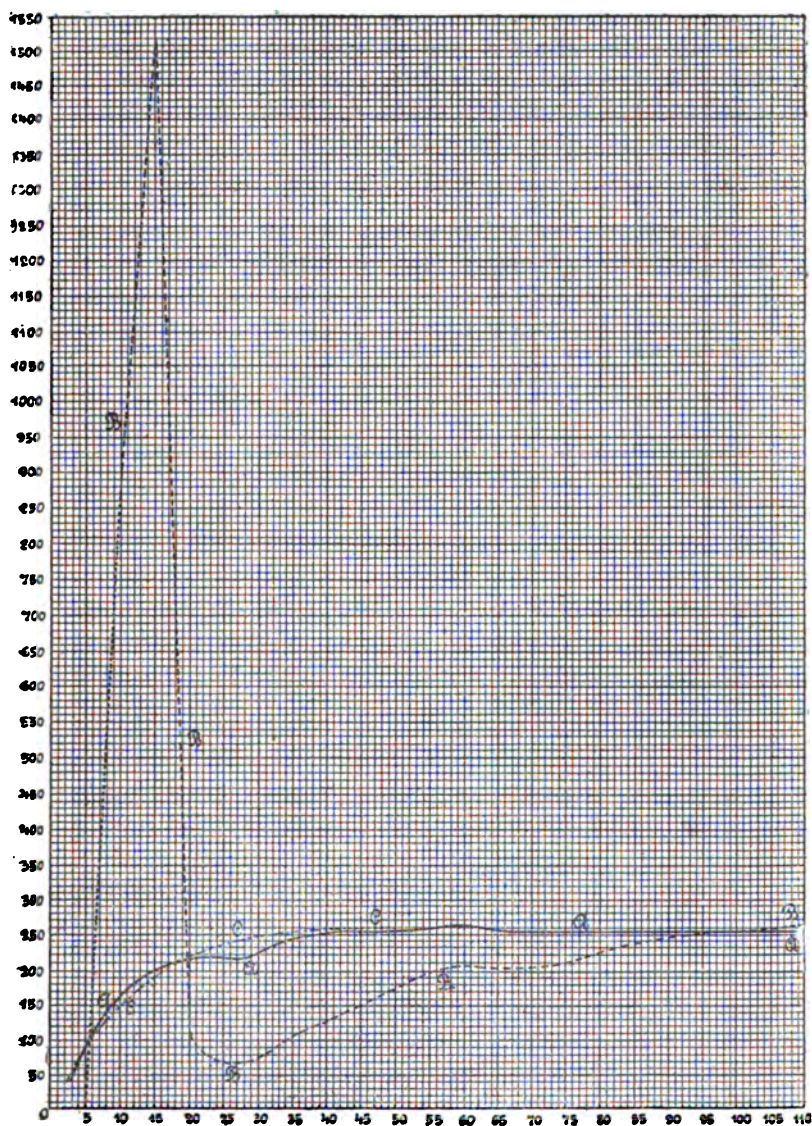
The results of this experiment are shown in Fig. 34. The curve marked A gives the temperature of the selenium, the curve B the strength of the current or the conductivity of the selenium, whilst curve C represents the calculated curve, according to which the temperature must have increased in the inner vessel, if no spontaneous alteration in the temperature of the selenium occurred. The axis of abscissæ represents the time elapsed since the immersion, the axis of ordinates simultaneously the temperature of the selenium in curve A, and its conductivity in curve B.

It follows from an examination of these curves that about $2\frac{1}{2}$ minutes elapsed before an increase in the temperature of the selenium became apparent. After a lapse of five minutes it has reached 80° , without deviating from the normal curve. Then it quickly passes above the normal curve, and continues considerably above it until the melting begins at 217° .

The maximum elevation of temperature of the selenium above that of the normal current takes place at about 170° , when it amounts to about 13° . From this point it again approaches the curve, cuts it at 215° , then maintains a pretty constant temperature for about fifteen minutes, again approaches the normal curve, at first quickly, then more slowly, without quite reaching it. This behaviour agrees with Hittorf's observation that amorphous selenium begins to change into crystalline at about 80° , and then parts altogether, or for the most part, with its latent heat. At about 170° this evolution of heat reaches its maximum, and the temperature of the selenium then increases more slowly than that of the normal curve. At 217° the selenium begins to melt, and heat is again absorbed by it, whence it happens that its temperature remains constant for about twenty minutes. It then again approaches the normal curve, at first quickly, then slowly, without quite reaching it.

During these changes in the temperature of the selenium very important changes in its conductivity are to be noted, as are represented by curve B. Five minutes after the immersion of the crucible, and therefore at a temperature of 80° , the selenium was

Fig. 34.



quite non-conductive. After ten minutes, when the temperature of the selenium was 162° , the deflection of the needle was 870 divisions of the scale; after another five minutes, at the temperature of the selenium of 200° , it was 152; and after another five minutes, at the temperature of 215° of the selenium, only 120 divisions of the scale. During the melting of the selenium now taking place the deflection fell to 70, then increased again with increasing temperature of the melted selenium, at first more quickly, and then more slowly, to 300. A limit of the rising of the conductivity could not be here observed after an interval of 140 minutes from the immersion of the crucible, although the temperature of the selenium remained constant after a lapse of sixty minutes.

The value of the figures in this series of experiments is only relative, as the thermometer bulb, being enclosed in the solidified selenium which conducts heat badly, must always have shown too low values with a temperature rising through the effect of heat from without; while, on the contrary, on the evolution of internal heat it could show higher temperatures than that of the selenium in proximity to the wall of the crucible, which was in contact with the carbon cylinders. They fully proved, however, Hittorf's observations, according to which amorphous selenium at about 80° C. begins to alter into crystalline selenium, then disengages a considerable quantity of heat, and becomes a conductor of electricity. It further proves Hittorf's statement that the conductivity of crystalline selenium increases with the temperature in increasing progression, and that it considerably diminishes with the taking up of the latent heat of fusion at constant temperature.

It further follows from these experiments that the conductivity of melted selenium increases with increase of temperature. I found from another series of experiments, in which a similar steatite crucible was heated directly by a flame, that the conductivity of melted selenium increases continually up to the temperature of 350° , at which a considerable evaporation occurred. A surprising phenomenon is that both with solid and molten selenium the conductivity diminishes with the duration of the heating, so that it conducts much better with quicker heating to a determined temperature than by slower heating to the same temperature; and,

further, that a rapid reduction of the conductivity is equally brought about by passing a continuous current through the heated selenium, as if a polarization took place opposing the passage of the current. For these reasons no closely agreeing numerical values could be derived from the numerous measurements of the temperature, and of the conductivity corresponding to it. As an instructive example of these experiments a diagram of curves is given in Figure 35, which brings into view pretty clearly the dependence of conductivity on temperature with many hours' continuous slow heating and cooling. The selenium contained in the steatite crucible was first made amorphous by cooling, then heated to 150° , and maintained many hours at this temperature, when it was slowly cooled. It must therefore have been crystalline selenium which had already given up its latent heat. Curve A shows the increase of conductivity with the increase of temperature indicated on the axis of abscissæ. The measurements were so made that by means of a Morse key a Daniell cell was maintained in a closed circuit formed of the selenium, the gas coke, and galvanometer wire until the deflection of the needle became a maximum. As the galvanometer was perfectly dead-beat this maximum deviation agreed perfectly with the continuous deflection. By freeing the key the Daniell cell was cut out of circuit. This method possessed the advantage, that simultaneously with the measurement of the current a measurement of the polarization sometimes occurring could be made. If, for instance, by bringing a directing magnet at a suitable distance below the magnet of the galvanometer, the directive force of the latter is made so great that a point is reached, but not exceeded, where the needle is perfectly dead-beat, as is the case with my galvanometer without a directing magnet, the mirror returns exactly to its zero position without diverging beyond it, both under the influence of the return current as well as when the current was broken. But if polarization is present so that a return current flows through the windings of the galvanometer during the return of the mirror, this current has an accelerating effect upon the magnet and brings it back beyond the zero position. The amount of this overstepping of the zero is a measure of the strength of the polarization. The polarization measurements mentioned below are carried out in this way when it is not stated that they are made with the continuous commu-

Fig. 37.

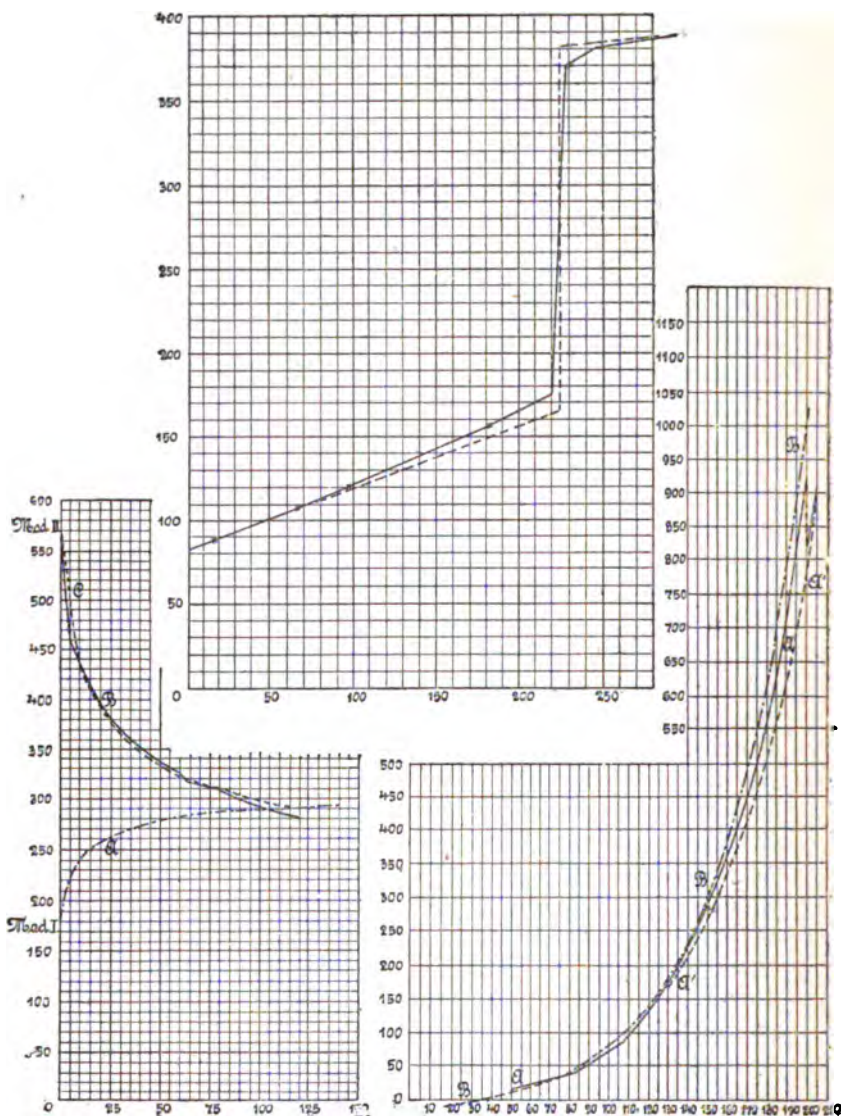


Fig. 36.

Fig. 35.

tator or without simultaneous current measurement from the zero position.

As follows from the appearance of the curve A A', the conductivity increases with increasing temperature in quicker progression. The first measurement at 50° gave 15; at 100°, 78; at 150°, 290; at 200°, 927. The bath was maintained at this temperature for fifteen minutes. The conductivity then sank to 819, and only rose again to 923 after further heating to 208°. On the temperature being again kept constant for fifty minutes, the conductivity sank again to 815. With the cooling which now commenced it was at 200°, 789; at 150°, 267; at 130°, 170, when the experiment had to be stopped. On the following day the experiment was repeated in the same way and gave similar curves, the curve A showing rising and curve A' falling temperatures. Dr. Frölich has sought to arrive at an empirical formula for the dependence of the conductivity on the temperature. Curve B is drawn from the formula found by him, viz., $k = C + a e^{at}$, or in figures $k = -17 + 8.48 (1.025)^t$. As $C = \frac{k}{-\infty}$, it follows that the conductivity at very low temperatures is—

$$k' = k - \frac{k}{-\infty} = a \cdot e^{at}$$

$$\frac{d k'}{d t} = a \cdot k'$$

that is, that increase of conductivity k' is proportional to k' itself.

The experiments described were carried out with electrodes of gas-coke in order to be certain that no combination of the melted or highly heated selenium took place with them. But after I had satisfied myself that neither platinum nor iron are acted upon by solid selenium, I employed for my further experiments the much more convenient spiral or grating of wire previously described, the spaces between which were filled with selenium.

I was most anxious to obtain a basis for the explanation of the important fact that amorphous selenium heated for a long time to 200° to 210°, so completely changes its physical properties that its conductivity at usual temperatures is thirty to fifty times greater than that of selenium made crystalline by heating to 100° to 150°, and then diminishes with increasing temperature, whilst that of

the latter increases. It appeared likely to me that this change must be intimately connected with the phenomenon that the conductivity of selenium diminishes at higher temperatures with the time of heating.

Two wire gratings with ten parallel wires 0.04mm thick, at a distance apart of 1mm, about 12mm in area, were filled with amorphous selenium between two mica-plates about 0.7mm apart. They were so arranged that the prolongation of the two grating wires projected from the paraffin bath in which they dipped, and could easily be connected with the galvanometer. The paraffin bath was then heated rapidly up to 200° C. Up to the temperature of 100° C. no current was observable between the two gratings with a battery of six cells. Then both began to conduct, and at 180° the current of a single Daniell could only be measured with the help of a shunt added to the galvanometer, which reduced its sensitiveness to $\frac{1}{10}$ th. At 200° the current of both gratings reached its maximum. Grating No. 1 had the conductivity 2,720; grating No. 2, 2,120. The temperature was now kept constant for four hours at 200°. After the first hour the conductivity of the first was reduced to 1,240; that of the second to 940. After the second hour the conductivity was respectively 1,090 and 820, and after the fourth 1,000 and 800. No. 2 was now suddenly cooled by dipping in cold petroleum, whilst No. 1 was cooled slowly, and during this time the conductivity was measured from time to time. The conductivity of the latter was at 180°, 1,020; at 150°, 2,460; at 130°, 5,730; at 120°, 8,320. At 100°, the mirror left the scale, and it required a greater shunt to the galvanometer which further reduced its sensitiveness. The conductivity was now at 100°, 17,020; at 80°, 21,280; and then slowly diminished. As the paraffin became solid at 60°, its temperature diminished very slowly from this point. When perfectly cool on another day its conductivity was only 6,190.

Grating No. 1 cooled quickly in cold petroleum from the temperature of 200° had a conductivity of 16,450 after cooling, and went back continuously, at first quickly, and then slowly. After an hour and a half it was 14,330, and next day 7,710.

It follows from these experiments that amorphous selenium heated to 200° for some time underwent a change by which its conductivity at this temperature was reduced to about $\frac{1}{3}$ of its

original amount. It has then the property of metals, that the conductivity increases with diminishing temperature, whilst that of crystalline selenium, which was not heated for a long time, diminishes rapidly with diminishing temperature.

On being quickly cooled to atmospheric temperature the selenium conducts about sixteen times better than at the temperature of 200° . But it does not retain this high conductivity permanently. It diminishes again by degrees, and only approaches a constant value after the lapse of many days.

If the heating of the selenium to 200° to 210° is continued until the conductivity is not reduced any further, the increase of the conductivity recommences with cooling. If this limit was not reached, the conductivity diminishes at first with continued cooling, and then approaches a turning point, after which it again increases. The height of this turning point depends on the duration of the heating, and the reduction of the conductivity so produced. If the heating continues only a short time, the character of the selenium is not thereby altered; its conductivity continues to diminish with reduction of temperature like the unchanged crystalline selenium.

The grating with which this latter was proved was dipped for eight minutes in a paraffin bath at 205° , and then quickly cooled by means of a current of air after being taken from the bath. Its conductivity fell during this time from 100, which it had 15 seconds after immersion, to 39.

Its conductivity fell quickly to 5 with cooling, and then rose again to 37. After being again heated for a quarter of an hour, the conductivity fell after cooling from 132, which it had already assumed in the paraffin bath at 212° , to 50, and then rose to 200. It did not, however, maintain this high conductivity, but sank by degrees to a low value.

It must here be observed that the remarkable behaviour of selenium which has been described of assuming the character of metallic conductors, by continued heating to 200° , was only observed when amorphous selenium was heated directly to 200° . If first heated for a long time to 100° , and thereby completely changed into simple crystalline selenium, on further continuous heating to 200° , the change did not occur at all or to a much smaller degree. Selenium, which has passed directly from the fluid condition

to the crystalline, which happens when selenium is melted and then kept very long at a temperature of 200° to 210° , is not metallically conductive as I formerly assumed, but behaves like crystalline selenium which has been changed at lower temperatures. This crystallization from the fluid condition takes place very slowly. A glass tube 6^{mm} wide, which was filled to $\frac{3}{4}$ of its length with selenium, was sealed and heated in a paraffin bath for an hour to the temperature of 230° . The temperature of the bath was then reduced to 205° , and with the help of a mechanical heat regulator uninterruptedly maintained for 24 hours at a temperature varying from 205° to 208° . On taking out the tube, the selenium in it appeared to have become quite solid. But when it was broken, after being quickly cooled, it showed that only the upper part of the mass was roughly crystalline, whilst the lower portion, about $\frac{1}{4}$ th of the whole mass, still existed as amorphous selenium. The lower portion of the bath in which the tube was placed upright, had probably become somewhat warmer than the upper, and so the crystallization had begun from above. The mass of the crystalline selenium was bubbly; it was perhaps owing to this that on opening the tube there was a strong smell of seleniuretted hydrogen. The selenium as well as the glass tube were quite air-tight, but no special precautions were taken to exclude steam.

From this rod of crystalline selenium a cylinder was cut, and this was held between two metal plates covered with a layer of copper amalgam. The resistance of this selenium cylinder, distinguished in the following table by A, was measured as well as that of another similar one of crystalline selenium, produced by heating amorphous selenium to 100° C. marked B, and of a third produced by heating amorphous selenium to 200° for 10 hours marked C, and the specific conductivity of their mass at 15° C, that of mercury being taken as the unit. These numbers have only a slight title to exactness as the individual determinations, especially of C, vary considerably.

	A (Mod. III.).	B (Mod. I.).	C (Mod. II.).
Conductivity	$\frac{1}{80,000 \text{ mill.}}$	$\frac{1}{1.4 \text{ bill.}}$	$\frac{1}{4,000 \text{ mill.}}$

In the following is always represented :—

Mod. I. Selenium treated like Cylinder B.

Mod. II. " " " " C.

Mod. III. " " " " A.

A very remarkable property of crystalline selenium of Mod. II. is that observed by Adams, that its conductivity increases with the E. M. F. of the battery used for its measurement. In the experiments of the following table selenium grating of Mod. II. was used, which had been modified by heating for a long time at 205°. The grating was immersed in petroleum, which was maintained at the constant temperature of 1·3° C by being surrounded with melting ice.

No. of Elements . .	1	2	3	4	5	6	7	8	9	1
Deflection of Mirror .	98	196	298	400	507	615	726	838	950	98
Calculated 98 n . .	98	196	294	392	490	588	686	784	882	
Difference . . .	0	0	4	8	17	27	40	54	68	

As the grating not much over 0·5^{mm} thick, with the two mica plates between which it was laid, was surrounded with petroleum of constant temperature, the measurements could not be very much influenced through heating by the current. As the conductivity of selenium modification II. is diminished by heating, the measured values with a greater number of cells might perhaps have come out too low. With higher temperatures the increase of conductivity with increasing E. M. F. is somewhat lower. When the same grating was maintained at 18° one obtained :—

No. of Elements . .	1	2	3	4	5	6	7	8
Deflection of Mirror .	113	228	343	459	578	698	818	938
Calculated . . .	113	226	339	452	565	678	791	904
Difference . . .	0	2	4	7	13	20	27	34

The absolute values of both series of experiments are not comparable, as the conductivity of the grating had changed on the following day and the galvanometer shunt was different. It hence appears that this peculiarity of selenium disappears more and more on approaching its neutral point, which with this grating lies between 30° and 40° . With selenium of modification I. which is not heated above 150° this phenomenon can only be established with very slight E. M. F. If heating and changing the conductivity with continuous currents are avoided the conductivity when 1 to 15 cells were used remains almost unchanged. As Adams's rod of selenium had the properties of modification II. probably because it was changed accidentally by very high temperature from amorphous selenium it is evident that he held the increase of conductivity with the employment of greater E. M. F. to be a general property of crystalline selenium.

The constancy of the E. M. F. of the cells employed, which both in these experiments and the later ones, were the constant Daniell batteries known as pulp cells, was proved before the experiments were made.

The fact has already been emphasized that the galvanic current alters the conductivity of selenium. This alteration always occurs in the same direction as if it was heated by the current. A continuous current therefore increases the conductivity of modification I. and diminishes that of modification II. But if the heating of the mass of selenium were the cause of the variation the variation must be proportional to the square of the strength of the current, and it must be much less when the gratings are maintained by their surroundings at a constant temperature. This is not however the case. The experiments were made with similar gratings of which one was modification I., the other modification II. Their results are graphically represented in figure 36. Both gratings were in petroleum at atmospheric temperature. Modification I. was continuously connected with the galvanometer wire through 12, modification II. through 3 Daniell cells. The axis of abscissæ gives the time in minutes of closing the current through the grating, the axis of ordinates the observed deflections of the mirror of which the position of rest was continually controlled. Curve A gives the conductivity of the grating of modification I., which was observed here as in the other curves

at each 5° variation of temperature. As is clear, the conductivity continually increases, at first quickly, but after a time more slowly, so that it appears to approach a constant value asymptotically.

The experiments represented in curves B and C are made with the gratings of modification II. and 3 cells, and in the series of experiments B the grating was kept in air of constant temperature, whilst in the series of experiments of curve C, which was carried out on the following day, it was immersed in petroleum of nearly the same temperature. As the E. M. F. of the battery employed was only $\frac{1}{4}$ of that with which the series of experiments of curve A was carried out, the ordinates of the first must be multiplied by 4, in order to be comparable with those of the last.

It follows from these curves that the conductivity continually diminishes, at first quickly and then slowly. The selenium nearly recovers its original conductivity a long time after the current ceases. If the heating of the mass of the selenium by the current was the cause of the reduction of the conductivity, there ought to have been a considerable difference between the curves B and C.

If the direction of the current through the selenium is reversed, after its conductivity is considerably reduced by the current, very different phenomena are observed and difficult to be determined beforehand, which appear to depend partly on the more or less complete change of the selenium in modifications I. or II., partly on the time which has elapsed since the change. Many gratings are only slightly affected by the current and do not manifest the least polarization, even when they are tested with a rapidly acting commutator. With others polarization appeared when the strength of the current exceeded a certain limit, and lastly with others it appeared with even a quite slight E. M. F. With these usually recently prepared gratings, the conductivity sinks by degrees with continuation of the current down to quite a small amount. If the direction of the current is now changed no polarization deflection is observed; the first deflection of the mirror is not greater than it was before the change; it soon begins to increase, however, and the current may in a few minutes reach a thousand times its first value. After passing the maximum the deflection diminishes again, and returns, if the current is kept on longer, slowly to its first slight value. It appeared at first, as if one had

to do here with Peltier effects called forth by the current by heating and cooling the touching surfaces between the selenium and the wires of the grating; in this way however the gradual increase of the current after the return is not to be explained. But one must remember the peculiar phenomenon described by Hittorf which he observed with sulphuret of copper. These are also to be observed with faulty submarine cables, when insulated with vulcanized india-rubber or gutta-percha, further with so-called unipolar conductors, such as soap, &c., and are here to be accounted for by an electrolytic change of these bodies at the place of contact and its neighbourhood.

The behaviour of the selenium is quite special, when a change of temperature occurs. As soon as its temperature varies, selenium immediately assumes a conductivity dependent on this temperature, which is greater with increasing temperature when it belongs to modification I. and less when to modification II. This conductivity is not however maintained. With modification II. it falls after every change of temperature whether this happened to be a rise or a fall of temperature, and approaches a limiting value at first quickly and afterwards slowly. The lower the temperature communicated to the selenium lies below the changing point, the greater is the conductivity it immediately assumes, the quicker and greater also is its diminution. If later on it is again brought to the higher temperature it gradually assumes the conductivity belonging to it, but if the difference of temperature was considerable, it does not again reach it fully. If the selenium has been long at the lower temperature and its conductivity has sunk to a minimum, the peculiar phenomenon may occur of a rise in temperature bringing about at the first instance a rise of conductivity with modification II., when the conductivity at the lower temperature had sunk below that belonging to the higher temperature. If the minimum has been reached for this temperature, this falls again lower than the minimum which happened at the lower temperature.

Very great cooling, a reduction of temperature to -15° for instance, appears quite to destroy the metallic property of modification II., or at least lowers the changing point below atmospheric temperature, so that after such a cooling a grating of modification II. shows the properties of modification I.

With gratings of modification I. these properties are more constant. The conductivity changes with increase as well as decrease of temperature, at first quickly then more slowly directly to the amount corresponding to the temperature.

There is a phenomenon still to be noticed, that is frequently met with in experiments with selenium, and caused very serious disturbances in mine until I happened to discover the cause of it, or at least to establish the conditions under which it occurred. Whilst as a rule, at least with old selenium gratings, in which the above described polarization no longer takes place, it does not matter as regards measuring the resistance in which direction the current is sent through the selenium, it sometimes happens that the measure of the resistance with one direction of the current is greater—sometimes even double as great—as with the other. It now appears that this remarkable phenomenon occurs when the surfaces of contact between the selenium and the leading wires are of very unequal size. When for experiments on light I had provided both sides of a selenium plate of modification II. about $\frac{1}{2}$ mm thick, with a frame of platinum wire 0.03 mm thick which on one side of the plate were 1 mm and on the other $\frac{1}{2}$ mm apart, the conductivity of the plate showed itself to be about twice as great, when the battery was inserted between the two wire gratings, so that the copper pole was connected to the side grating consisting of 10 parallel wires, the zinc pole with the narrow grating of 20 wires, $\frac{1}{2}$ mm distance apart, as with the inverse arrangement.

With two double gratings prepared as nearly as possible alike of the kinds described as A and B, the conductivities of the specified arrangements were—

	Copper Pole to the 10 Wire Grating.	Copper Pole to the 20 Wire Grating.	Ratio.
Double grating A . . .	490	244	0.49
Double grating B . . .	282	192	0.67

It hence appears as if the resistance of the selenium was here quite dependent on the magnitude of the positive anode. The phenomenon observed earlier with equally large conductive plates is hence explained simply by both not being in equal internal conductive connection with the selenium mass. Polarization did not occur in all these cases.

This phenomenon has not occurred with modification I., and that much larger grained and better conducting selenium crystallized from liquid selenium which we have called modification III.

From the experiments described it is evident that crystalline selenium differs essentially in its behaviour as regards heat and electricity from other simple bodies. Tellurium and carbon have equally with it the irregular property of conducting electricity better at higher temperatures, whereas all the other simple bodies conducting electricity, *i.e.* the metals, conduct better at lower temperatures. Selenium however does not possess this property at all temperatures, but loses it by continual heating to 200° C, and then acts as regards electricity just like a metal, *i.e.* its conductivity increases with cooling. This metallic condition of amorphous selenium transformed at higher temperatures to the crystalline condition is however not stable. It slowly transforms itself with and after the cooling into the non-metallic but electrically conducting selenium, becoming crystalline at lower temperature, until the residue remains dissolved, the amount of which depends on the temperature to which it was cooled. As selenium is a simple body, it cannot be merely chemical combinations or changes which bring about these different conditions, but we are easily led to the assumption, that there is a third allotropic condition, which solid selenium takes by continued heating to 200° C., a condition which is only stable at this temperature, and at lower temperatures is only protected from more complete destruction and transformation into electrolytically conducting selenium by its being dissolved in it or combined with it. The occurrence of a critical point is thus explained, on passing which the metallike condition is converted into one having an electrolytic character; as well as its decrease with time and with a fall of temperature. This view is supported by many other phenomena and analogies.

It has been pointed out by Arndsen,* and frequently proved since, that the conductivity of a pure and solid metal rises in an almost direct line from the absolute zero of temperature until it nearly reaches its melting point, or otherwise expressed, the specific resistance of a pure solid metal is equivalent to the absolute quantity of heat the metal contains. Simple metals in the solid condition can-

* Pogg. Ann., Vol. CIV., p. 1 and Vol. CV. p. 148.

not possess latent heat, and it is not improbable that it is just in this that the condition of metallic conduction is to be sought. It has been shown by Matthiessen * for potassium and sodium, and by me † directly for tin and indirectly for copper, silver and zinc, that with the taking up of the latent heat of fusion, a sudden increase of conductive resistance takes place. This increase begins already to a slight extent before the temperature of fusion, and continues after the beginning of the liquid state, which may perhaps be explained by a melting already begun and not yet completed. According to my previous experiments which are graphically given in Fig. 37 for tin, the increase of resistance of tin after taking up the latent heat of fusion would correspond somewhat to that called forth by a rise of temperature of about 511°C . Rudberg ‡ gives the latent heat of fusion of tin at 13·814, Person § at 14·25. If one assumes the specific heat of tin to be 0·051, the absolute quantity of heat in tin near its melting point amounts to about 25·3 units, not taking into consideration the alteration of specific heat near the melting point, and the quantity of heat depending on the melting, a rise of temperature of only 259° should take place. According to this, as regards tin, latent and free heat do not increase the conductive resistance equally, but the influence of latent is nearly twice as great as that of sensible heat.

If according to this we cannot be allowed to extend Arndsen's proposition, that the conductive resistance of pure metals in general, therefore also in the heated condition, is equivalent to the absolute quantity of heat, there yet remains the most characteristic circumstance as regards metals, that their conductive resistance increases both with temperature and the latent heat they take up. This also occurs with alloys. That the resistance of the so-called chemical alloys is greater than that of the separate metals from which they are formed is proved by their giving up latent heat on solidifying as Rudberg || and others have shown.

By assuming the above definition for metals, selenium and tellurium, and especially other simple bodies such as carbon, which conduct electricity without decomposition, but whose resistance

* Pogg. Ann., Vol. C., p. 177.

† Pogg. Ann., Vol. CLXXXIX., p. 99.

‡ Pogg. Ann., Vol. XIX., p. 133.

§ Pogg. Ann., Vol. CXLVI., p. 300.

|| Pogg. Ann., Vol. CXXI., p. 460.

diminishes with rising temperature, cannot be reckoned among them. As selenium however after long heating to 200° , conducts metallically at this temperature, it must in this condition be considered as a metal. The change which occurs, as under similar circumstances with phosphorus, can only be sought in a giving up of latent heat. It must therefore be assumed that selenium both in the crystalline and amorphous condition is an allotropic modification of the metallic, *i.e.*, selenium free from latent heat, and that it differs from the true metals essentially in that, they only take up latent heat in changing their aggregate condition, but the first also at all temperatures below 200° .

It may also be supposed that tellurium and carbon change similarly. Perhaps later experiments will show, that all the simple bodies which do not conduct electricity are allotropic modifications of their simple radicals, not capable of existing by themselves, *i.e.* maintain their latent heat in the solid condition and are on this account, like amorphous selenium, non-conductors of electricity.

It is not however quite distinctly shown by this theory, how it is that the bodies, standing on the border between metals and metalloids such as selenium, tellurium and carbon, conduct better with increasing temperature, although the sums of the contained heat are greater. As however with selenium, the force with which it holds the latent heat taken up in the solid condition evidently diminishes with rising temperature, for it begins to give up part of it already at 80° , and the rest at 200° , one can assume that the electric current overcomes the resistance opposed to it through the latent heat all the easier the smaller this force and consequently the higher the temperature.*

* The statement that a force exists in bodies by which they retain specific heat more or less, is only to be taken figuratively. According to the mechanical theory of heat, the phenomenon that bodies take or give up heat at certain temperatures, whether connected or not with alteration in their state of aggregation or density, can only be imagined, when the elements of the bodies have an altered position to one another, to bring about which more or less internal work is expended in the negative or positive direction, which then shows itself as appearing or disappearing free heat. To these different molecular conditions a certain stability must be ascribed, diminishing with increasing temperature. If the temperature is reached at which the condition is no longer stable, a new position of equilibrium of the body's elements occurs, which is stable again between certain limits of temperature. The metallic condition of a solid body would, therefore, be that in bringing about which no force is consumed, a condition which is only stable with metals, and which enables them

Now to explain from this point of view the peculiar and contradictory phenomena, which were observed especially with modification II., which according to it is to be considered as a solution of metallic in crystalline selenium, one must assume that an essential part of the resistance of the selenium has its seat at the boundary layers of the surfaces of contact and that these layers are altered electrolytically by the electric current. This alteration may consist sometimes in the metallic selenium being separated from the crystalline, and thereby temporarily or permanently destroyed, and changed into crystalline or amorphous. By reversal of the current by temperature and by time which all operate a gradual change of this slightly stable condition, a reproduction or other transformation can be brought about, by which the conductivity again alters.

An exact examination and definition of the special causes of these phenomena require experiments much more detailed and tedious than it has been possible for me to make. They have at least been brought together in a sort of causal dependence, and a basis has thus been found for an explanation of the obscure phenomenon, that the conductivity of selenium increases with light, a phenomenon which gave rise to this paper, and will form the subject of its continuation.

In conclusion I have to thank Dr. Frölich and Dr. Obach for their valuable assistance in making the numerous and often difficult experiments, of which only the smallest part could be included above.

to conduct electricity, and in such a manner that the resistance to conductivity is proportional to the absolute temperature.

Electrolytic conduction would then be explained by electricity in its passage causing the metallic molecular condition, which can be effected so much more easily and perfectly, the less stable the molecular condition and the higher the temperature. As melted metals still conduct electricity metallically, it must be assumed that with the melting the metallic character of the sides of the elements of the bodies in juxtaposition is not lost, as is the case with the allotropic modification without alteration of its aggregate condition.

ON THE DEPENDENCE OF THE ELECTRIC CONDUCTIVITY OF SELENIUM ON LIGHT AND HEAT.*

ON the 17th February, 1876, I communicated to the Academy the first part of this inquiry, which was limited to a description of the alterations, which selenium experiences under the influence of heat and of the electric current. As I did not succeed in discovering in other bodies, the influence of light on the electric conductivity of selenium previously described by others as well as myself, I was obliged to consider this phenomenon as closely connected with the special properties of selenium, and a searching inquiry into it appeared to me to be the only way to obtain an explanation of this remarkable action of light.

Unfortunately my engagements in other business have so far made it impossible for me to complete the experiments on the influence of light on selenium which had at that time been in great part made.

In the meantime a work by Dr. Richard Börnstein of Heidelberg has appeared under the title "On the Influence of Light on the Electrical Resistance of Metals," which calls in question the foundation of my work, in that Dr. Börnstein undertakes to prove that the increase of the conductivity of metal with light is not restricted to selenium, but takes place with tellurium, platinum, gold and silver, and probably all other metals.

In my experiments on the influence of light upon other metals I always endeavoured to obtain the greatest possible sensibility by the choice of methods and instruments, and had started from the same principle as Börnstein, viz., to make the illuminated surfaces as large as possible in proportion to the thickness; but I was always led to the conclusion that any increase of conductivity must stand in a certain ratio to the specific conductivity of the metal concerned. Since selenium of the form described by me as modification II., in which it not only conducts electricity best, but is at the same time most sensitive to light, conducts about 240,000

* Monthly Report of the Berlin Academy of Sciences of 7 June, 1877.

million times worse than silver, the increase in the conductivity of a thin piece of metal must be easily discoverable with slightly sensitive instruments, if the increase of conductivity of the illuminated surface of the metal was dependent on its specific conductivity.

The matter, however, stands otherwise, if it is assumed that by the action of light on the outer surface of the metal, a conducting layer is produced, the conductivity of which does not stand in any direct relation to the specific conductivity of the illuminated metal itself, and therefore probably does not conduct better with good conducting metals than that produced on the surface of the selenium. As the conductivity of the developed conducting layer can only be measured, as an increase of the conductivity of the illuminated metal and a limit is put to the reduction of its thickness by the necessary cohesion of the metal plate, with good conducting metals the limit of the difference which can be no longer perceived with the most sensitive instruments is soon reached. A selenium plate, such, for instance, as used by me in my experiments and in the selenium photometer consists of eleven parallel wires, 0.1^{mm} thick, 10^{mm} in length, and 1^{mm} distance apart, and has thus a resistance of about one million mercury units. The selenium can therefore be supposed to be replaced by a mercury layer connecting the parallel wires conductively of the thickness x given by the formula

$$1000000 = \frac{1}{100} \cdot \frac{1}{1000} \cdot \frac{1}{x}$$

or,

$$x = \frac{1}{100000 \text{ millions}} = \frac{1}{10^{11}} \text{ mm.}$$

With an illumination which doubles the conductivity of the selenium plate, the developed conducting illuminated layer may be replaced by a layer of mercury of similar thickness.

The gold leaf used by Dr. Börnstein in his experiments in which he found by the bridge method an increase of conductivity of 0.0001, had a resistance of three mercury units, a length of 24^{mm} and a breadth of 9^{mm}. Hence if the gold leaf is replaced by a layer of mercury of the thickness of y , we have for y

$$3 = \frac{24}{9y} 0.001 \text{ or } y = \frac{89}{10^6} \text{ mm.}$$

If the conductivity of the gold leaf increased 0.0001 upon illumination, as Dr. Börnstein found, the illuminated layer hereby developed must correspond to a layer of mercury of 0.0001 of this thickness and therefore of $\frac{89}{10^6}$ mm. ; the illuminating effect was therefore about 8,900 times as great as with selenium if it is assumed that the illumination used by Dr. Börnstein would have doubled the conductivity of the selenium plate. In order to explain the action of light on selenium by an illuminated layer similar for all metals, the conductivity of the Börnstein gold leaf need be increased by only $\frac{1}{8}$ millionth of its value, an amount which cannot be determined experimentally. Tellurium is the most likely metal to afford evidence on this point since its conductivity is only 0.00042 that of gold, if one could succeed in producing tellurium in as thin conducting layers as gold leaf.

The grounds on which I refused the hypothesis of a conducting illuminated layer as making its appearance in all metals, do not therefore depend on the negative results of my efforts to show the sensitiveness to light of other bodies than selenium, but essentially on the sensitiveness to light of selenium being in a high degree dependent on its purity and molecular condition. The least admixture with other metals reduces its sensitiveness to light in a very great degree. When I added only a half per cent. of silver to the selenium used for the selenium plates, no further sensitiveness whatever to light was perceptible. By too strong an action of light, by great cooling or heating, the sensitiveness to light is influenced in a high degree, even when no essential alteration occurs in the conductivity of the preparation itself. All this would only be explicable with difficulty, if a conducting surface was produced on the upper surface of the selenium by the action of the light, which was independent of the conductor lying under it. The origin of such an illuminating conducting layer, could only be explained in general by the assumption that the gases condensed on the upper surface of the metal were so modified chemically by the action of the light that they become conductive, and that with the discontinuance of the illumination a re-aggregation into the non-conducting condition would take place. But in that case a layer of selenium firmly attached to glass or

mica by fusion, must show hardly any or a still smaller sensitiveness to light than one exposed to air; this is not however the case, as is testified by the construction of my selenium preparations sensitive to light, melted between mica plates.

Whilst however I was strengthened by these reflections in the opinion, that sensitiveness to light is a specific property of certain selenium modifications and is not shown by other bodies, it yet appeared to me not impossible that more sensitive methods and instruments than I employed, might show a sensitiveness to light in other metals. Experiment could alone decide this.

In Dr. Börnstein's work, besides some erroneous statements regarding my inquiry—to which I shall refer later—some of his results appeared to me very astounding. For instance, he observes a somewhat greater increase in the conductivity of a platinum wire

of 0.00022^{mm} thickness than with a gold leaf of $\frac{19}{1000000}^{\text{mm}}$.

thickness, although the proportion of the projection of the illuminated surface to the section of the metal in the two cases was as 2348 : 1. If this were right, the layer sensitive to light must conduct 2,000 times better with platinum than with gold, which at all events does not appear likely. Equally surprising is the enormous difference of the sensitiveness to light which is given by measurement with the bridge method and with Weber's damping method. Whilst the bridge measurement gave an increase in conductivity of about 0.01 per cent., the damping method gave under similar circumstances an increase of conductivity of from 3 to 5 per cent., which was therefore in this case from 300 times to 500 times as great as in the first. Dr. Börnstein supposed that this great difference in the results of his measurements was due to the currents generated in the windings of the wire by the swinging magnet having been much feebler than those of the Leclanché cells with which he made the bridge measurements, and he founded on this the statement "that the diminution of the conductivity produced by the electric current (which he described as electric after effects) was accompanied with a diminution of sensitiveness to light." What were the electromotive forces generated in the windings by the swinging magnet must remain uncertain, for no calculation is practicable as the data given by Dr. Börnstein are not complete enough. Anyhow, so great a dependence of the

action of light on the strength of the current is opposed to the experience gained with selenium.

If Dr. Börnstein's view were correct that the direct comparison of the resistance gave undoubtedly a so much smaller result than the measurement of the resistance by means of the damping method, because the action of the light was concealed or diminished by the heating of the illuminated thin metal plates by the current and by the simultaneous diminution of the sensitiveness to light, then at all events direct resistance measurements with very little E. M. F. must have given similar results to those obtained by the damping method. I therefore placed my galvanometer with deadbeat swinging bell magnets, and scale at 8 metres distance, with which the earlier experiments were carried out by a galvanometer with an astatic pair of two little bell magnets, which were fastened to an aluminium wire at a distance of about 100^{mm}. Each magnet was in the centre of a coil with approximately 445 convolutions of wire 1^{mm} thick of 1.84 units resistance. At the upper end of the aluminium wire was fastened a Steinheil's light mirror of 9^{mm} diameter which was protected from air currents by a casing with windows. Any desired directive force could be given to the magnet system by means of a rotatable bar magnet fixed at a given distance below the magnets which also served to fix the zero at the middle of the scale 1 metre long with millimetre divisions placed at eight metres distance as before. This specially delicate galvanometer was connected up to a bridge the four branches of which, the metal plate to be tested being one, were made as nearly as possible alike, and as little different as possible from the resistance of the galvanometer. Between the two variable bridge branches of German silver wire was inserted a German silver wire of 300^{mm} length and 3 units resistance, stretched round the periphery of a round slate disc provided with a graduated circle, on which a platinum roller with index and vernier could be moved. The platinum roller was connected with one pole of a Daniell cell, the resistance of which was brought to 10 units by inserting a wire resistance. By means of a resistance box this cell could be closed through a conveniently large shunt. The working E. M. F. in the nearly equal branches of the bridge E' was then $E \frac{w'}{w + w'}$, if w was the resistance, E the E. M. F. of the cells, and w' the resist-

ance of the shunt. To be able to control exactly the sensitiveness of the measurement there was inserted in the branch of the bridge containing the metal leaf to be tested a copper wire of 0.001 unit resistance, which could be short circuited by a short, thick, amalgamated copper link, with the help of two little mercury cups. If a perfect equilibrium was effected by repeated short circuitings of the working battery at first weak, and then increased to the strength of a Daniell cell, the taking out or putting in of the resistance of 0.001 unit produced a deflection of the needle of about 20 divisions of the scale; therefore, variations in the conductivity of one bridge branch of 0.0001 unit, could be still observed with great exactness.

The objects which I tested were thin gold films spread over glass plates, metallically soldered at the ends by means of melted Rose metal to tinfoil coverings and to the connecting wires, further very thin translucent deposits of gold, platinum, and silver prepared in different ways which were soldered in the same way to the terminals, and last the thinnest possible plates of aluminium and tellurium. These preparations were inserted in the respective bridge branch whilst they were protected from the action of light by means of a cardboard box placed over them. After equilibrium had been obtained, and a certain time had elapsed, the battery contact was made, and after the usual slight deflection of the mirror had been read, the cardboard box was removed. The metal plate was then exposed to the illumination of a petroleum lamp placed in a lantern with a wide slit, the rays of which passed through a glass vessel of cylindrical shape 12^{cm} in diameter, filled with a concentrated solution of alum, and were thus concentrated on the metal plate, whilst the heat rays were absorbed by the alum solution. The cardboard box was repeatedly put on and taken off, whilst the battery continued closed. In almost all cases, a slow feeble heating of the metal plate took place owing to the action of the current and the illumination, but never any clear evidence of a reduction of the conductivity through the action of the light. Unfortunately it turned out that the galvanometer with this degree of sensitiveness could not be maintained steady enough to enable reliable measurements to be made so as to decide the question whether in general a measurable light effect was found in other metals besides selenium. The galvanometer itself could not

be sufficiently protected from external currents, nor could the thermo current be sufficiently excluded which with such slight resistances and electromotive forces appeared to be very disturbing without special precaution.

I obtained a similar negative result with a different arrangement of my experiments. The metal plate to be experimented on was inserted directly in the galvanometer circuit. If an effective electromotive force E' of 0.01 Daniell was inserted in the circuit the spot of light went off the scale; it was brought back again to the middle of the scale by adjusting a magnet bar to the galvanometer in the usual way. When this was once done, the cross wires of my telescope on closing the circuit, even after a long period of rest, pointed to the same scale division without oscillations owing to the galvanometer being perfectly dead beat. At this moment the cardboard box was removed by an assistant and the metal plate exposed to the light. Even in this way no undoubted effect of light could be observed on any of the above-mentioned metal plates, although a reduction of the resistance by 0.0001 must have been observed with the greatest certainty. If Dr. Börnstein's opinion was correct, that so great an increase in the action of the light occurred through a diminution of the E. M. F. as he had found by the use of the damping method, this ought to have been observed to a much greater extent with the use of 0.01 Daniell than with 1 Leclanché cell which he used in the bridge measurement.

I was unable, for the reasons already mentioned, to increase still further the sensitiveness of the galvanometer employed, and could only seek to further strengthen the action of the light by the use of metal plates as thin as possible but still certain to be conducting. In fact by means of known methods, exceedingly thin, yet conducting metal films were placed on glass plates, and securely connected to conducting wires. The last could only be perfectly effected by galvanically silvering or gilding the glass strip provided with the thin metal film, in a solution of hyposulphite of silver or gold, a diagonal strip having been protected from the silvering by a layer of lac afterwards dissolved off by ether or alcohol. In this way a very good conducting film of gold could be produced, which by reflected light looked like a beautiful gold mirror, but allowed daylight to shine through not

with a green but with a light blue colour. The resistance of this gold mirror 15^{mm} long and 10^{mm} broad, was 7,000 units after repeated and constant measurements. Putting the conductivity of gold as equal to 34 (that of pure mercury being = 1) the thickness of the gold film may be taken as 0.0000000063^{mm} if so thin a film conducts similarly to a mass of metal.* Even with this preparation I could not perceive any action of the light, although on account of the great resistance I provided my galvanometer with 400 convolutions of thin wire, the resistance being 7613 mercury units, and had thereby much increased its sensitiveness. It is remarkable, however, that the resistance of this excessively thin film of gold was quite constant with an E. M. F. of 0.01 Daniell, and did not manifest the effects of the current observed by Dr. Börnstein.

As it occurred to me to submit my negative results to a control by other experimenters, and it also appeared to me of importance to find out by the application of much more sensitive methods than Dr. Börnstein or I had been able to employ, whether the action of light is generally manifested with other metals besides selenium, I induced my friend Gustav Hansemann to undertake experiments in his laboratory which is arranged for experimenting upon weak thermo-currents. In Hansemann's laboratory a comparatively dark space is divided off by means of a partition of thick panes of plate-glass, which separates the observer from the instruments, which are set up in the space so that all air currents and other causes of local changes of temperature may be avoided. The necessary movements were produced by cords, which passed in through the glass partition. This, and the great sensitiveness of his mirror galvanometer, having convolutions of wire of 0.5 of a mercury unit resistance, made it possible for him to employ as a source of electricity an iron-copper thermo-element, which gave a constant E. M. F. of about 0.001 Daniell, when one junction was maintained at a constant temperature by means of boiling water, and the other by a current of water from the water supply. With this slight E. M. F. there could no longer be any question of a concealment of the action of the light by the warming of the metal

* But this cannot be assumed as the surface is non-reflecting and therefore rough.

plate and by secondary action of the current, and it was to be assumed that the 300 to 500 times greater illuminating effect obtained by Dr. Börnstein by means of the damping method would certainly be made evident if it were not founded on error. As Dr. Hansemann has described his experiments in an appendix to this communication, I will here only state that he was no more able than I to observe any effect due to light. Dr. Hansemann also obtained no positive result by means of the damping method, by which he sought to reproduce Börnstein's remarkable experimental results by the use of a suitable mirror galvanometer, which I had placed at his disposal for the purpose, when the necessary care was taken to guard against the appearance of thermo-currents and other disturbances.

What may be the causes of Dr. Börnstein's peculiar experimental results cannot be explained, as his experiments are not described with sufficient minuteness. With measurements of this kind, which necessitate the use of instruments of the greatest sensitiveness, disturbances may easily occur with a certain constancy, and it is always somewhat risky to base new fundamental phenomena upon mean values exclusively, especially when the result lies well within the limit of error of the several experiments, as is the case with Börnstein's experiments.

From the above, I cannot accept the conclusions which Dr. Börnstein draws from his experiments, but, on the contrary, I must maintain my opinion that with the means hitherto at our disposal the action of light is not noticeable with other metals than selenium.

I will not deny the possibility that this may yet take place in the future with very elaborated methods of measurement, and then too the action of light on selenium may be explained by this general action of light, but I do not consider that we are authorized in considering it as existing until proved by unimpeachable experiments. Until then we must look upon the action of light on selenium as belonging exclusively to selenium, and seek in its special qualities an explanation of this action of light.

Before I pass on to this, I must briefly return to certain allegations of Dr. Börnstein on my experiments brought before the Academy on the effect on selenium of light and the electric current.

Dr. Börnstein has¹ repeatedly brought forward statements which refer only to the precise experiment described, as though they were generally binding experimental results. Thus the law attributed to me that the conductivity of amorphous selenium, but not its sensitiveness to light, increases with the duration of heating is not universally correct. In the same way it is not correct that a polarization current always shows itself as a consequence of continuous currents through selenium. I have, on the contrary, clearly stated that this is only true in exceptional cases, with strong currents, and freshly prepared selenium plates of the well-conducting modification II., and that in most cases even with the most sensitive arrangements no polarization was observable. I explained this polarization as an electrolysis of the contact surfaces between the selenium and the conductors bounding it. Dr. Börnstein assumes the sensitiveness of tellurium to light to be a fact, without having himself observed it, although I have definitely called it in question. He depends exclusively on the solitary experiment of Professor Adams, who believed he had noticed the action of light on a piece of tellurium an inch in length.

As, according to Matthiessen, tellurium has about 2400 times the specific resistance of gold, and has besides many physical properties in common with selenium, it is not at all unlikely that tellurium is sensitive to light under certain circumstances. But its specific resistance is only about a millionth of that of selenium, and as on account of its brittleness it cannot up to now be produced in the form of thin plates like the ductile metals, it will be difficult under ordinary circumstances to show its sensitiveness to light. I have not succeeded in showing this with plates about 0.01^{mm} thick, which were pressed out of melted tellurium by a heavy pressure between heated glass-plates.

I have already stated in my previous communication to the Academy in 1875, that the increase in conductivity of selenium by illumination is nearly in the ratio of the square root of the strength of the light. Before undertaking a closer investigation of this question I sought first to ascertain, that the same intensity of the same coloured light with the same selenium preparation under exactly similar conditions shows exactly the same effect. These experiments should at the same time decide the ques-

tion, whether selenium would be suitable for the construction of a useful photometer, which would then have this great advantage over those formerly used, that it would be free from the personal errors of the observers which so vitiate photometric measurements, and would also give determined numerical values for the comparison of lights of different colours.

The selenium preparations used for these experiments were the same as those described in the first part of this enquiry. They consisted of two platinum, steel, or copper wires 0.05 to 0.10^{mm} thick, which, insulated from one another, were so fixed on a plate of mica, that a space of 0.5 to 1^{mm} remained unoccupied between the wires. They were fastened by means of two rows of fine holes at a distance apart of about 10^{mm} with which the mica plate was provided. The wires were drawn through these holes, and the ends so joined up that there was a wire grating on the upper surface of the mica plate, the wires of which were connected alternately with one or other of the two conducting wires. On this grating a layer of amorphous selenium was placed about $\frac{1}{2}$ a millimetre thick, on which a second plate of mica was laid, and this firmly fastened to the first plate of mica. Then the whole was confined between two small metal plates with elastic pressure, and dipped into a paraffin bath, brought to a temperature of 200° to 210° C., and maintained for many hours at this temperature by a suitable heat regulator. After cooling occurred, the plate had as a rule a resistance of 500,000 to 1,500,000 mercury units, and a sensitiveness to light which corresponded to an increase in conductivity by diffused daylight of 0.2 to 0.5; the sensitiveness to light and the conductivity fell generally after a few days to about one-half. Such a selenium plate was now fastened to the base of a metal tube about 30^{mm} wide, and 60^{mm} long, and the conducting wires connected to insulated terminals fixed outside it. The tube itself could be rotated about a vertical axis, so that by turning the tube the selenium plate could be surely and quickly directed to one source of light or another. A wooden rod 1 metre long with millimetre divisions was so fixed to the stand which carried the axis, that the axis coincided with the beginning of the divisions. A candle socket provided with an index was movable along the wooden rod, and served to hold the standard candle used for comparing the sources of light measured.

To carry out the measurement the apparatus was so arranged that the graduated rod with the standard candle formed a right angle with the source of light to be measured, so that by turning the tube quickly from one position to the other the selenium could be exposed to the action of one or other source of light without appreciable loss of time. The terminals of the tube were then joined up to the leading wires of a sensitive galvanometer, in the circuit of which a suitable galvanic battery could be inserted by means of a key. According to the sensitiveness to light of the selenium plate and the sensitiveness of the galvanometer, 1 to 10 Daniell cells were inserted, and in certain cases even stronger batteries. Four standard candles were first arranged near to one another, at a distance of 100^{cm} from the selenium plate, and the standard candle placed on the slide was approached until by quickly turning the selenium tube from one source of light to the other no permanent change in the deflection of the mirror occurred, even if the short moment of darkness during the passing of the tube from the one position to the other produced a short backward motion of the spot of light. The position of the index gave the distance of the standard candle as 49.1^{cm} instead of 50^{cm}, which it ought to have been according to the inverse square of the distance. The cause of this difference evidently was the intensifying of the flames of the four candles standing near to one another due to reciprocal heating.

In a further experiment a very regularly burning petroleum lamp, placed in a closed internally blackened case with a diaphragm, was balanced at different distances against the standard candle, the flame of which was maintained at a height of 24^{mm} by frequently snuffing the wick.

Distance of the lamp .	100	150	200	250	300
Distance of the normal candle with similar deflection of the mirror . }	33.7	51.4	69.3	81.0	92.6
Calculated strength of the light of the lamp in normal candles . }	8.8	8.5	8.3	9.5	10.5

The variations in the calculated intensity of light are explicable by unavoidable changes in the brightness of the standard candle; at the further distances the illumination of the walls of the room through the open burning standard candle becomes very considerable, by which the illuminating value of the latter would be increased.

To obviate this defect, two petroleum lamps with shades were arranged at different distances, and the distance of one continually altered until there was equilibrium.

Distance in Metres of the		Ratio of the Square of the Distances.	Difference.
English Duplex Petroleum Lamp in the Casing.	Petroleum Lamp in the Casing.		
6	1·890	10·07	+ 0·09
5·5	1·775	9·58	- 0·40
5	1·615	9·60	- 0·38
4·5	1·495	10·10	+ 0·12
4	1·290	9·60	- 0·38
3·5	1·090	10·50	+ 0·52
3	0·930	10·40	+ 0·42
Mean 9·98			

Doubtless greater care employed in these experiments would lead to much more conformable results. I was in this case contented to prove by the experiment, that the selenium photometer without special care gives sufficiently exact comparative results to be applicable as a practically serviceable photometer.

When I first commenced my experiments with selenium, I hoped that it would have been possible to construct a photometer with its help, which could have given the intensity of the light directly, and for this purpose I sought to obtain definite relations between the intensity of light, and the increase in conductivity of selenium. It appeared, however, that its conductivity depends upon too

many uncontrollable factors to be used directly as a measure of light. In particular the duration of the illumination as well as the intensity of the light appeared to be an active factor. With modification I. continuous illumination produced a progressive increase of the conductivity, whilst with modification II. the conductivity had already reached its maximum after a short time, often after 5 or 10 seconds, and then diminished again, at first more quickly and afterwards more slowly.

This property of increase or diminution of conductivity due to the duration of the illumination occurs in very different degrees with different selenium preparations. The more carefully the

TABLE A. (Mod. I.)

The measurements are made with 12 Daniell cells, which brought about a deflection of 92 intervals of the scale previously to illumination.

Minutes . . .	0	2.5	5	10	15	20	25	30	35	40	45	50	55	60
Deflection . . .	92	112	132	152	162	167	173	177	180	183	185	187	189	190
Action of light . .		20	40	60	70	75	81	85	88	91	93	95	97	98
Differences . . .			40	20	10	5	6	4	3	3	2	2	2	1

heating of the selenium above 100° C. has been prevented during its change from the amorphous to the crystalline condition, the smaller is its conductivity, and the more slowly it increases with the duration of the illumination. The selenium plate used in the first of the accompanying series of experiments denoted by A was converted by immersion in a petroleum bath heated to 100° C., whilst the plate used in the series denoted by B was slowly heated in the petroleum bath up to 100° C., and then kept for many hours at this temperature. The experiments were so carried out that a sharp image about 14^{mm} long was thrown on the selenium plate through a lens set up before the diaphragm opening of a brightly burning petroleum lamp. The dark heat rays were absorbed to a great extent by means of a glass trough 3.5^{cm} thick, filled with a solution of alum. The electric current passed only during the measurement through the selenium preparation, and only until the mirror of the dead-beat galvanometer had reached its position of rest.

On the following day both plates had nearly the same conductivity in the dark as before the experiment. As may be seen the light acted much more slowly on the selenium plates of the second series of experiments which conducted much worse in the dark, so that it only reached its maximum after an interval of six hours. The great irregularities are probably consequences of the variation in temperature. The temperature of the room rose during the experiments from 21° to 25° C.

The behaviour is quite different with continuous illumination of the selenium, which was converted into the crystalline condition at

TABLE B. (Mod. I.)

The measurements are made with 50 Daniell elements.

Time	0	5'	10'	15'	30'	1 ^h	2 ^h	3 ^h	4 ^h	5 ^h	6 ^h	7 ^h	7 ^h 30'
Deflection	160	162	167	173	191	196	200	212	228	235	244	235	229
Action of light . .		2	7	13	31	36	40	52	68	75	84	75	69
Differences						36	4	12	17	6	9	-9	-7

a temperature from 200° to 210° C., and then kept for a long time at this temperature. The measurements collected in the following table are made with a plate of modification II. in the manner above described. 1 Daniell was employed for that purpose, and it was inserted each time long enough for the deflection to reach its maximum, which was generally the case in about 10 seconds. The unilluminated selenium plate gave a deflection of 85 divisions of the scale.

TABLE C. (Mod. II.)

Duration of Illumination.	Deflection from Illumination.	Differences.	Duration of Illumination.	Deflection from Illumination.	Differences.
10"	148	148	0 ^h 40'	76	-2
0 ^h 5'	117	-31	0 ^h 45'	74	-2
0 ^h 10'	104	-13	0 ^h 50'	72	-2
0 ^h 15'	96	-8	0 ^h 55'	70	-2
0 ^h 20'	90	-6	1 ^h	69	-1
0 ^h 25'	86	-4	1 ^h 5'	68	-2
0 ^h 30'	82	-4	1 ^h 10'	66	-1
0 ^h 35'	78	-4	1 ^h 15'	65	-1

After many hours darkness the deflection returned to 32 on the scale.

It follows from these experiments that both modifications of selenium are distinguished, for one thing by very different conductivity, but specially because modification II. reaches its maximum conductivity after a few seconds interval, while the selenium converted at a lower temperature only does so after some time. When this maximum is reached the action of the light begins to diminish again—an occurrence which may be described as fatigue of the selenium—and approaches a minimum asymptotically with modification II. To what extent this return happens with modification I. was not enquired into ; but the diminution of the action of the light after passing the maximum, appears to take place just as slowly as its rise to the maximum.

This influence of the duration of the illumination on the amount of the action of the light, which varies with each preparation of selenium, makes it difficult as already said to establish definite relations between the intensity and action of the light. The numerous and varied experiments which I have conducted on this point did not give sufficiently concordant results. They only showed that the action of light increases in a smaller proportion than the square root of the intensity. The experiments were first carried out so that two constant sources of light could be compared at different distances in a rising and descending series. Further a movable thin plate with holes as nearly as possible 1, 2, 3 to 6^{mm} in diameter was placed before the large bright flame of an English duplex lamp with flat wicks, and the selenium preparation repeatedly exposed to illumination through these holes in rising and descending series. If the law of squares were correct, the action of light should have been proportional to the diameters of the holes. A third method gave the most accordant and reliable results, which consisted in dividing a ray of light by means of a double prism into two rays, and exposing the selenium plate alternately to one or other pencil of rays alone or both together. For these experiments the duplex petroleum lamp was used with a diaphragm opening 2^{mm} in diameter. The selenium gave a deflection of 50 divisions of the scale in the dark with 4 Daniell cells.

selenium converted at 100° C., it increases with rising temperature as with carbon. On this account the first which I have called modification II. conducts much better than modification I.*

Modification II. may now be considered as a mixture or compound of crystalline and metallic selenium. A complete conversion into metallic selenium is not possible, as the latter in the pure condition at the usual temperature of the air is not in a stable condition, and on cooling is converted back into crystalline selenium with an absorption of latent heat, until a remnant only remains by admixture or combination with crystalline selenium. The behaviour of ozone is quite analogous. When pure oxygen is submitted to gaseous electrolysis by means of the ozone apparatus elsewhere described by me, a portion of the oxygen is converted into ozone. If the ozone formed is continuously removed by a silver plate or other means from the mixture of oxygen and ozone produced by the process, the whole of the oxygen can be converted by degrees. If, on the other hand, the ozone produced is not removed, a limit is soon arrived at, when ozone is no longer produced, since only a certain quantity of ozone is protected by admixture with inactive oxygen from reconversion into this latter. Probably ozone is an allotropic modification of oxygen free from latent heat, and could be regarded as metallic oxygen like the hypothetical metallic selenium. In this "condition of freedom from latent heat," or "metallic condition" bodies have the greatest affinity for forming chemical combinations, and it is probably to be generally considered as the so-called active condition of the body such as it appears in the nascent state. As heat diminishes the stability of that allotropic condition of bodies in which they contain latent heat, this view seems to explain the pretty generally observed intensification of chemical changes by heating. In the same way the generally observed fact is made clear, that electrolytic conduction is improved by heating, for it must be assumed, that the chemical combinations of different bodies assume allotro-

* To obtain this pure, amorphous selenium in thin plates it must be heated to about 100° C. in petroleum or other heat-conducting fluid, and maintained for a long time at this temperature. If this precaution is not taken the selenium converted into thick pieces is heated to such a degree by the disengagement of latent heat, that a still further disengagement of latent heat takes place, and therefore a partial conversion into modification II. Many apparent contradictions in the results of different experimentalists may be thus explained.

pic molecular conditions retaining latent heat, which must be brought back into the metallic condition before they can enter into new combinations. The fact that even simple bodies, like carbon, tellurium, selenium, conduct like electrolytes, as they conduct better with increase of temperature, would then prove that with this conduction an electrolytic action actually takes place, and would, therefore, separate at one electrode metallic selenium for instance, and at the other a higher allotropic modification or one retaining more latent heat, of which the first at least is not stable in the pure condition at ordinary temperatures, and, therefore, is converted after the cessation of the current, or perhaps even during its continuance, by re-absorbing latent heat. In a similar manner the chemical action of light is to be so regarded that the æther vibrations of the chemical rays of light break up the stability of the allotropic molecular condition "retaining latent heat," and thereby restore the active or metallic conditions of the illuminated molecules of the body.

According to this theory the action of light on selenium is to be explained by ascribing to the light rays which strike the surface of the selenium, and penetrate to a very slight depth into it, a similar action to that which high temperatures exercise upon it. They reduce crystalline to metallic selenium, which conducts very much better and sets free the latent heat of the former. On cessation of the illumination, the metallic surface of the selenium reverts to crystalline selenium as the metallic condition is only stable under illumination or at a high temperature. It is not, however, yet made clear, that this action is brought about essentially only by the visible rays of the spectrum, and not by the chemical and dark heat rays which lie outside of the visible spectrum. But probably later experiments may show, that to each body corresponds a certain period of vibration of the æther waves which produces on it the maximum chemical action of light, or that the diminution of the stability of allotropic modifications of simple bodies is brought about most powerfully by æther vibrations of medium wave length, that of compound bodies most by æther vibrations of small wave length.

That the action of the light on the better conducting modification II. which already contains metallic selenium dissolved in it, takes place much more quickly and is much greater than on the

unmixed crystalline selenium is made clear partly, because with the first there is a smaller quantity of crystalline selenium to be reduced in order to produce a conducting metallic surface, but partly also because the good conducting surface is directly connected with the conducting wires at a few points only. There is almost all over a film of selenium unconverted into the metallic condition which has to be traversed by the current, and on the resistance of which the strength of the current is dependent.

To explain the important phenomenon of the fatigue of selenium with continuous action of the light it must be assumed that crystalline selenium is more translucent than metallic. In this case the action of light will extend at first to greater depths, and convert badly conducting crystalline selenium into good conducting metallic selenium. But as soon as the surface of the selenium has become a continuous metallic film, it acts as a screen which keeps back the light from the metallic molecules at first converted at greater depth, and so allows them to revert into crystalline selenium. With simple crystalline selenium this fatigue does not seem to occur, on the contrary its conductivity increases by illumination for many hours as was shown earlier. But the full action of the light actually occurs very much more slowly, for after many hours' illumination the maximum action of the light is reached, and then a falling off of the conductivity is also noticed.

That the action of light is restricted to the surface of the selenium and to the layers lying next to it can be easily shown by comparing the action of light on the two sides of a selenium plate. In consequence of its construction the wire grating touches the surface of the plate on the one side, whilst the other side of the grating is covered by a thin layer of selenium. If the former side is illuminated the action of the light is 2 or 3 times as great as when the latter is illuminated.

There yet remains to discuss the varying action of coloured light rays and their disturbing influence on the comparison of differently coloured lights by the selenium photometer.

I have found Sale's statement to be correct, that the action of light commences with the visible violet rays of the spectrum, then increases pretty regularly to the red, is still found in the ultra red, but not in the rays lying beyond it. The following series of experiments was made with a narrow selenium plate consisting of

only two parallel platinum wires at 1^{mm} distance apart, and 4 Daniell cells. The spectrum was produced by a glass prism and a brightly burning petroleum lamp with a slit.

This series of experiments performed without special care and only to obtain general information, shows sufficiently well that the selenium photometer cannot be used as it is for the comparison of different coloured lights.

	Dark.	Violet.	Blue.	Green.	Yellow.	Red.	Ultra-red.	Dark.
Deflection .	139	148	158	165	170	188	180	150
Action of light	0	9	19	26	39	49	41	11
Differences .		9	10	7	13	10	-8	-30

This leads to the question as to what is really meant by the photometric comparison of differently coloured lights. A comparison of the sensation of light produced by means of our organs of sight is impracticable and quite individual. Light does not serve us, however, to perceive a greater or less brilliancy, but to enable us to clearly distinguish or recognize distant objects, and a correct photometer should show differently coloured lights as equivalent when they make distant objects perceptible to us in the same degree. This property does not entirely coincide with the conception of equal brilliancy. If one looks at a landscape alternately through blue or yellow glass it appears much brighter in the latter case; but it is not for this reason impossible if the yellow glass absorbed much light, that the objects of the landscape might be much more clearly distinguished through the blue glass.

The blue light which reaches our eyes gives us in this case a better definition, even though it produces a feebler sensation of brilliancy. A photometer intended for practical use should give the illuminating value of coloured light as thus defined.

The photometers constructed up to the present time which are based on the production of an equal sensation of brightness, are entirely unsuited for this purpose. Even setting aside the different illuminating value of coloured light, it is not possible to form a decided opinion thereon, when two differently coloured lights are equally bright. In any case such a judgment is altogether sub-

jective. The selenium photometer has certainly the great superiority over these photometers, that it gives undoubted results of the action of light of all colours; these results, however, are not directly applicable as the selenium is affected by differently coloured light in a different degree. Besides, the ascertainment and use of a scale for the action of light of different colours of the spectrum for the correction of results with the selenium photometer is not sufficient, because it is not absolutely settled what lighting value the coloured rays of the spectrum have. If however a scale were ascertained for the purpose, it could only have a very restricted value, for it would not be applicable for the comparison of the lighting value of coloured lights from terrestrial sources.

I have sought to fix by empirical means a scale of the lighting value of differently coloured lights which have the same lighting effect on selenium.

Small print on white paper was looked at through a telescope placed at a distance of about 5 metres. A petroleum lamp burning uniformly and with a tolerably white flame, could be brought closer to the print by means of a cord worked by the observer until the print was just legible in the otherwise dark room. This process was repeated with a coloured glass plate placed before the lamp. If the lamp were brought so near that the type was again just legible, then both lights had the same illuminating power. If now the light action on a selenium plate brought into the plane of the paper was each time determined, a factor was to be found in this proportion of the action of the light, with which the results of the selenium photometer for equal lighting value of this coloured light were to be multiplied. In this way the co-efficients for all colours of the spectrum were to be obtained, and thus a correction table for the comparison of different coloured lights was to be produced. Unfortunately, it happened, however, that the eyes of the different observers were so unequally and to such a degree affected by the exertion of deciphering the printing with the weak light, and especially also through the sudden change of the colour of the light, that no concordant results could be obtained, and the experiments had to be given up. It is to be hoped that other observers with better means will succeed in producing such a correction table for similar lighting values of coloured light. The sensitiveness of selenium to light would then serve as a photo-

meter, which unlike former photometers that only compare colourless or similarly coloured light could compare lights of all colours free from the personal error of the observer.

Yet even without such a correction table, the selenium photometer has the advantage over others, that it does not, like these, lead to false results with slight variations in the colour of the light, but gives definite results, the significance of which may be agreed upon.

ON THE ELECTROMOTIVE ACTION OF ILLUMINATED SELENIUM DISCOVERED BY MR. FRITTS OF NEW YORK.*

MR. CH. FRITTS of New York sent me last summer, a description of his method of making selenium plates sensitive to light, which is different from mine in many points, and enclosed some of the plates prepared by him. These do not consist like mine of parallel platinum wires, embedded in a thin layer of selenium, but of a thin homogeneous layer of selenium, which is laid on a metal plate, and after being heated (so as to convert the amorphous into crystalline selenium) was covered with fine gold-leaf. Mr. Fritts has found that the green light transmitted through the gold-leaf on further passing through the selenium improves its conductivity. In fact, the conductivity of the selenium plate between the metallic base plate and the gold-leaf is increased from 20 to 200 times in some of the preparations sent over, when the gold-leaf is illuminated by direct sunlight falling upon it perpendicularly. Even when illuminated by diffused daylight the action of the light is greater with Mr. Fritts's preparations than with mine. One of the plates sent over to me showed hardly any sensitiveness to light, but possessed another highly important

* Monthly Reports of the Berlin Academy of Sciences of 13 May, 1875, and 7 June, 1877.

property, namely, that a galvanometer inserted between the gold-leaf and the base-plate indicates an electric current in the direction of the motion of the light through the selenium as long as the gold-leaf is illuminated. I supposed at first that this current was not permanent, but was of the nature of a polarization current, which only continued until the molecular modification of the selenium was completed by the illumination, and the first experiment appeared to support this assumption. Later and more searching experiments have however convinced me that this supposition was erroneous. We have in fact to do in this case with a quite new physical phenomenon, which is of the greatest scientific importance. It follows from my experiments, that a difference of potential arises between the base-plate and the gold-leaf, when the latter is illuminated, which is to all appearance proportional to the strength of the light, and which continues unchanged so long as the illumination continues. Dark heat rays are not electromotive, we are therefore prohibited from assuming a thermo-electric action as the explanation of the phenomenon. Mr. Fritts assumes that the light waves penetrating into the selenium are changed directly into an electric current, and the proportionality of the strength of the current to that of the light does indeed lead to this conclusion. This was proved approximately by means of the series of experiments in the following Table :—

Strength of light in standard candles .	6.4	9.9	12.8	16.8
Strength of current .	18	30	40	48
Quotient	2.8	3	3.1	2.8

The power of the light was measured by means of a Bunsen photometer, the strength of the current by the deflections of a sensitive mirror galvanometer. When the gold-leaf was exposed to the light from the S.E. quarter of a cloudless sky, whilst the sun itself was concealed by lofty buildings in the neighbourhood, there resulted the measurements collected in the following Table :—

Time of obser- vation . . }	9 ^h 37 ^m	10 ^h 5 ^m	10 ^h 30 ^m	11 ^h	11 ^h 35 ^m	12 ^h	12 ^h 30 ^m
Deflection of the galvano- meter . . }	190	196	209	223	250	250	244
Time of obser- vation . . }	1 ^h	1 ^h 30 ^m	2 ^h	2 ^h 30 ^m	3 ^h	3 ^h 30 ^m	4 ^h
Deflection of the galvano- meter . . }	245	249	288	188	173	172	108

It follows from these that the E.M.F. of the selenium plate increased almost uniformly from 9.30 A.M. to 11.35 A.M., then remained constant for nearly two hours with slight variations, and then diminished nearly uniformly until 3 o'clock.

Mr. Fritts was unable to give any explanation of the cause of some of his selenium plates improving in conductivity when illuminated, whilst others acted electromotively. He complained of the uncertainty of the construction of the plates, of which it was impossible to foresee the properties, and gives different manipulations by which inactive plates can sometimes be made serviceable. Fundamental experiments are therefore still required to determine on what the electromotive light action of many selenium plates depends. Yet the existence of a single selenium plate with a property such as that described, is a fact of the greatest scientific importance, for here, for the first time, we meet with the direct conversion of light energy into electrical energy.

PHYSICAL AND MECHANICAL CONSIDERATIONS,
SUGGESTED BY THE OBSERVATION OF AN
ERUPTION OF VESUVIUS IN MAY, 1878.*

DURING my stay in Naples in May of this year, Vesuvius wore a crown of vapour which now and then during calms rose to about one-third of its height above sea level. During the night the crown of vapour appeared slightly luminous. It seemed remarkable to me, that viewed with a good telescope, it appeared to consist of rings of vapour following quickly upon one another. The glow was not constant ; its brightness was very variable, and now and then it appeared to be intermittent.

When on the 14th of May I climbed the very difficult ascent up to the edge of the old crater, I was very much surprised at the view which presented itself. At the highest point of the cone of ashes which rose in the middle of the great crater up to about half the height of its edge, there was to be seen a brightly glowing opening, from which violent explosions took place in pretty regular sequence every two or three seconds. The force of these explosions could be approximately measured, by a quantity of red hot stones and pieces of lava being hurled up by them considerably above the point where I was standing on the edge of the old crater, and after the almost perpendicular fall which followed, they rolled down the upper surface of the inner cone of ashes. The brightly glowing mouth of the active crater formed an irregular square, the average length of the sides of which I estimated at five to six metres. Each explosion carried the surrounding air along with it, and thus formed above the top of the mountain a ring of vapour rotating round itself from within outwards and expanding as it rose. It was accompanied by a dull report, which perceptibly shook the whole top of the mountain. The appearance of flames was not observed. When, however, there was bright sunshine the mass of expelled vapour had in the neighbourhood of the mouth of the crater the yellow colour which slightly luminous flame usually assumes in sunshine.

* Monthly Report of the Berlin Academy of Sciences of 17 Oct. 1878.

This imposing spectacle differed considerably from the notion I had formed of volcanic action from written descriptions. These short sharp sort of explosions of expelled vapour following at such short intervals of time were not to be explained on the assumption that masses of steam, taking their rise in the fluid interior of the earth, or produced by evaporation of water entangled in the up-rushing lava, had burst through the lava in the channels of the crater owing to its overwhelming tension. A bubble of gas or steam which rises up through overlying liquids can either rise up slowly exactly in a similar way to a bubble of air in water, whilst its volume continually increases in proportion to the diminution of pressure, and then it quits the liquid without excess of pressure; or, if the high pressure suddenly arises and overbalances the pressure of the liquid enclosed in a narrow channel, it must throw out the liquid in a continuous mass. In the first case, the bursting up of each bubble of steam must have occasioned a quiet and therefore non-explosive kind of vaporisation, in the latter on the contrary with each explosion great masses of lava would have been hurled up, and it would have required more time, before a following explosion could have occurred after the crater channels had again become filled with lava. There is, however, hardly any ground for imagining that such a sudden preponderating pressure of steam should occur in the glowing depths. If we also assume that masses of water enclosed in the lava or magma rise up with it in the crater channels, and pass into the form of steam after corresponding reduction in pressure, this change can never be sudden, as the pressure only diminishes slowly with the diminishing depth, and as the water to pass into the form of steam must take up latent heat, by which it and the surrounding lava is cooled, therefore the cause of the vaporisation will be removed until the cooling produced by the steam formed is replaced through heat conduction from the distant portions of lava.

There is yet another possible cause of the sudden evolution of a preponderating steam pressure which may be here discussed. At a very high temperature the elements of water are dissociated like other chemical compounds. It might then be assumed that the magma contains not water, but its dissociated elements, condensed H_2O , and that it is burnt again to form water when the temperature, through reduction of the column of pressure, and

with it the compression of the magma, was reduced to a certain degree. It is however in the highest degree improbable that under the powerful pressure which the solid crust of the earth exerts on the magma, a dissociation of the water can occur, since the pressure promotes the combination of the gases to denser water.

Earlier experiments have proved to me that at a very high pressure cold mixed gases explode and are converted into steam.* But if it were assumed that the dissociating force of the temperature exceeded the associating force of the pressure, that consequently the water may be contained in the magma in the form of compressed explosive gases, even then it must not be assumed that a sudden combination of the elements of water into steam united with a much further heating could occur, because the consequent greater heating must again bring about dissociation; the process could therefore only proceed slowly.

It now only remains to assume that hydrogen or the combustible combinations of hydrogen rise up in the crater, which, mixed up in some way with oxygen to form an explosive mixture, were afterwards exploded in the upper portion of the channel of the crater. Whence did the combustible gas originate, whence the oxygen, and how was the necessary mixture formed in so short a space of time?

* I tried the experiment in the following manner: Glass tubes about 50^{cm} long, and of about 1½^{cm} internal, and 4 to 5^{cm} external diameter, were closed at one end by fusion, and filled for the greater part with acidulated water. Two stout covered platinum wires of about 15^{cm} length were then fixed in the open end, the upright tube at this end was surrounded with a paper cover, which was filled full with the well known mechanical cement formed of kolophonium and wax fused together, after the air in the open end of the tube had been somewhat expanded by heating it. The cement was then drawn some centimetres into the tube on cooling and perfectly closed it. If now the tube was placed in a wooden box somewhat on a slant so that the fluid completely surrounded the platinum wires, and then a battery of 10 Daniell cells was connected up to the ends of the platinum wires, the water began at once to be decomposed. If only 3 or 4 Daniell cells were used, the decomposition of the water ceased in a short time and only began again when the number of cells was increased. If more powerful batteries were used an explosion occurred regularly after from 10 to 30 minutes with the appearance of flame which shattered the tube. The light was observed in a mirror, which was placed before an opening in the box. The phenomenon was always repeated under similar circumstances with perfect regularity; therefore the pressure alone could be the cause of the explosion of the mixed gases. I have not determined the amount of pressure necessary to produce explosion at a given temperature. According to calculation a glass tube similar to those used should bear a pressure of about 2,000 atmospheres, but I do not think that the pressure of the gas before the explosion amounted to half this.

Only after a long contemplation of the interesting spectacle did I make an observation which explained the last-mentioned occurrence, the admixture of the rising combustible gas with oxygen. Little clouds frequently separated from the clouds of steam cast up, which then moved quickly sideways and returned with great speed into the crater. Shortly afterwards the next explosion occurred. The mechanical occurrence which at first seemed so mysterious, was made quite clear by this observation, which was confirmed by my companions. If it be assumed that a continual stream of combustible gas breaks forth from the crater either empty to a great depth or filled with loose detritus, this gas once ignited would burn with the oxygen of the air as a powerful though only slightly luminous flame. At the beginning of the volcanic action the crater would be filled with atmospheric air. If now a continual eruption of lava takes place, and with it a streaming out of combustible gas, this hot and light gas will mix very quickly with the cold and heavy atmospheric air found above it, and form an explosive mixture with it, which is then ignited by means of red-hot pieces of lava thrown up with it. The consequence may be a powerful explosion such as is often observed at the commencement of a period of outbreak. If the crater's mouth is wide and open so that the atmosphere has easy access to the interior, this first explosion would often not be followed by another, but the outflowing combustible gas would burn quietly in the depth of the crater, with the heavy air continually flowing in. If on the contrary, as is the case at Vesuvius, the mouth of the crater is narrow, so that no simultaneously inpouring and outpouring of the gas and air can take place, then all the conditions for a series of explosions are present. The highly-heated steam produced by the first explosion is ejected to a great extent at a high velocity from the opening. The next moment two forces work together to produce a relative vacuum in the crater. On the one hand the steam still remaining in the upper part will be carried forward in consequence of the inertia of its mass if atmospheric equilibrium has already occurred, and thus a relative void be produced in the crater, and on the other hand the cold air entering after the explosion will thereby partly condense the steam still remaining, and thus cause a further inflow of atmospheric air. This inflowing air must now mix the more quickly with the com-

bustible gas regularly streaming out, since the heavier air is above the light and both are in active motion. As soon as the mixture becomes explosive, the second explosion occurs and so forth. A great number of such explosions must impart the high temperature thus obtained to the walls of the mouth of the crater and bring them to a red heat. Probably the bright glow which is observed at the mouth of the crater only proceeds from these continuous explosions, and it is hence not improbable that the channel of the crater was at a glowing heat only at a considerable depth in the interior of the earth, whilst the mean depths were dark. The interval of time between the explosions must depend principally on the extent of the air-filled space in the crater. It may hence be supposed that an acceleration in the sequence of the explosions points to a rise of lava in the crater and therefore to an approaching outbreak.*

A more difficult question than that of the mixture of the combustible gases with atmospheric air is the nature and source of the combustible gases, which rise through the crater from the interior of the earth, and what forces heave up the melted masses to the summit of a volcano on an eruption.

The considerable production of steam makes it very likely that hydrogen has been principally burnt, but leaves it undecided whether the hydrogen was free or combined with other combustible substances—such as sulphur, carbon, &c. Perhaps the combustible gas was also much mixed with steam, which could then partly form the clouds of vapour. Without doubt these contain considerable quantities of sulphurous acid. When the wind drove the steam, mixed to a great extent with atmospheric air, to where I was standing, I had to change my position very speedily, for breathing the sulphurous acid was insupportable. Sulphuretted hydrogen, which burns with oxygen to sulphurous acid, could only have been produced by the decomposition of water in the deeper layers of the solid crust of the earth. If the course of the crater passes through considerable layers of pyrites perhaps full of fissures, then heated steam released from the fluid interior of the earth, *i.e.*, the magma, and rising through the channel of the crater must decompose the pyrites, and produce sulphuretted

* This actually soon occurred.

hydrogen, which then rises up mixed with undecomposed steam. The same would happen if ejected red-hot magma penetrates fissures filled with rain or sea-water. But although the activity of Vesuvius may perhaps be thus explained, it cannot be assumed that the explanation will serve for all volcanoes, for the products of combustion of many of them contain no sulphurous acid or very little, and therefore it can hardly be assumed that strata containing sulphur or iron pyrites are to be found under all volcanoes. Sulphuretted hydrogen and marsh gas are decomposed at high temperatures under slight pressures. However, it is not quite certain that at the high pressure under which the magma exists, they cannot subsist in it notwithstanding its high temperature; but decomposition must happen in any case, when the pressure diminishes owing to their ascending with or through the magma. That the magma contains water and hydrogen is proved for the former at any rate; nor is this surprising, if one starts from Kant and Laplace's theory of the formation of the world. According to this it is to be assumed that the atoms were in the beginning scattered singly through the space of the universe. If centres of attraction were formed—perhaps through unequal distribution—they would move towards these. According to the mechanical theory of heat, as Helmholtz has shown, the momentum accumulated in the accelerated moving atoms must be transformed into heat on impact, and the temperature must rise in rapid progression with the progressive condensation. With the rise of temperature chemical affinities would come into play. Related atoms coming into contact must group themselves into molecules, which, perhaps by further contacts, and increased temperature due to still greater condensation, separated and recombined to form again other combinations. All chemical combinations must arise which would be possible with the prevailing conditions of temperature and pressure, and must exist in intimate mixture in the magma or earth mass, which has become fluid by heat and pressure.

The usual starting point of geological assumptions that the earth was a fused ball consisting mainly of silicates, and that water together with gases must have surrounded it as a glowing atmosphere, does not agree with the above view. Water and gas could only have escaped at once in the gaseous state from the outer layers of the earth becoming fluid, which were under a low

pressure, whilst in the greater depths of the magma they must have remained partly dissolved, partly retained in intimate mixture with it. Against the assumption that hydrogen and other combustible material remain behind in the magma, the fact may be mentioned, that oxygen now forms a great portion of our atmosphere and must therefore have existed in excess. We know, however, much too little of the effect of the prodigious pressure, and the high temperature corresponding to it, which prevailed in the interior of the earth at its formation, and of the alteration in the force of affinity, produced by later cooling, to be able to decide whether the oxygen was not wholly combined, at the formation of the earth, and was not first released at later periods from the then fluid magma together with the greater part of the water now found on the earth's surface. The fact that the sun's atmosphere consists for the most part of free hydrogen, according to the results of spectrum analysis, and that considerable masses of hydrogen still break forth from the solar centre, point to there being a surplus of hydrogen in the solar spectrum, and is therefore favourable to the last view. That we no longer meet with free hydrogen in our atmosphere might perhaps be explained by the fact that hydrogen which is specifically lighter, and compressible within far wider limits, must form a much higher atmosphere than the heavy gases, and therefore must be almost entirely withdrawn from the earth, and that its limit exceeds the limit of equilibrium between attraction and centrifugal force. We know that water heated to a high point under pressure dissolves quartz and silicates to a considerable extent, as well as that on the other hand molten silicates absorb both water and so-called permanent gases. We do not know how far these properties may be increased by the enormous pressure and high temperature in the interior of the earth. It is probable that highly-heated water saturated with silicates, and silicates saturated with water existed uncombined both in intimate mixture with each other, and may in part still exist. The same thing will hold good for carbonic acid, the aqueous solution of which takes up chalk and magnesia in considerable quantity under high pressure. Forces now arise in this non-homogeneous mass, which in course of time must produce a separation of the uncombined fluid masses lying close to one another. Gravity must have gradually conveyed the specifically

heavier to the depths of the earth and therefore moved the lighter to the periphery, whilst on the other hand the more powerful attraction of the heavy masses must have repelled the lighter in the same way as air-bubbles repel each other in liquids. The result of these forces working very slowly, especially in viscous fluids, the first most active of which however diminishes with the intensity of gravity as the depth increases, must have been a quite gradual separation of the heavy liquids from the light, and an aggregation and motion of the latter towards the periphery. But *a priori* with the formation of the ball of the earth, heavy and light regions of matter containing more alkalies, carbonic acid and water must have been formed, because the matter was originally spread not uniformly, but in groups through the universe.

To this grouping of heavy and light masses in the interior of the earth, or this "mud-making" as Reyer calls it, must be ascribed an important part in the formation of the crust of the earth as well as in the volcanic phenomena now taking place. Before I go more closely into this I must, however, consider the important grounds which Sir William Thomson opposes to the assumption that the earth may yet be liquid in its interior, or even may still have been liquid at the formation of the first solid crust.

Thomson asserts that the earth must have a much greater rigidity than a massive glass or even steel ball, as otherwise the tides could not take place to the extent they do. Were the interior of the earth still liquid, the earth and water must mutually accomplish the tidal motion, no relative rise of the water could consequently take place. The slight resistance of a moderately thick crust can in no way affect this. Were the earth a solid glass ball its elasticity would still permit of a tidal motion, which would reduce the tide calculated for a perfectly rigid earth, and corresponding so nearly with experience, to $\frac{2}{3}$ of its value or to $\frac{1}{2}$ if it were of steel. He therefore considers it impossible that a solid crust could have been formed before the whole earth had become rigid, as the solid rocks according to Bischof's researches are 20 per cent. specifically heavier than the molten mass from which they have solidified. Thomson, therefore, assumes that the earth has been a solid nucleus with a deep sea of molten silicates, which covered it. When on further cooling solidified masses of rock formed on the surface, they would have sunk down to the solid nucleus. Only after

the whole molten sea had been filled in this way with masses of rock, could a lasting solid crust have been formed. The spaces between the sunken masses remained filled with the molten masses, and are in part still so. From these fluid lava masses enclosed in the solid body of the earth, arises according to Thomson's view the lava of the volcanoes, and the cause of earthquakes is the falling of masses of rock from the roofs of such hollow spaces on to their floors. William Thomson bases this opinion upon a calculation of his brother, James Thomson, according to which the point of solidification of liquid masses is altered by pressure differently accordingly as the body expands or contracts in solidifying. This calculation has been fully proved for ice. Starting from Bischof's experiments and the hypothesis of Laplace, according to which the increase of the square of the density is proportional to the increase of the pressure, Thomson calculates that for the interior of the earth, the temperature of fusion of silicates has been always much higher than that due to compression. As the distribution of mass in the body of the earth calculated in this manner corresponds to that necessary for bringing about the observed precession and nutation, W. Thomson maintains the correctness of Laplace's hypothesis, and therefore also that his view of the nature and formation of the earth is proved. Mallet, Roth, and other geologists have opposed this on geological grounds. Mallet also questions the correctness of Bischof's experiments, and has found from his own experiments that blast furnace slags contract only six per cent. in solidifying from the temperature of fusion to the solid state. Experiments which my brother Frederick Siemens has undertaken in his glass bottle works at Dresden at my request, explain these great differences in experimental results. It has been found that the limpid melted bottle glass very rich in quartz, contracted very quickly from a certain determined temperature, and thereby became viscous. The further the cooling advanced the less was the contraction, and there was a still smaller contraction when solidification itself took place from the yet plastic mass of glass, than in the case of solid glass with the same difference of temperature, which is equivalent to a slight expansion in passing to the solid state. It is easy to prove the considerable contraction which takes place in cooling thin liquid glass, from what occurs on filling a crucible in the furnace up to the brim with a quantity of refined

bubble-free glass, and then taking it from the oven. The level of the mass of glass then sank visibly, at first quickly then more slowly, although the contraction of the wall of the crucible which cooled in the first place must have exercised an opposing influence. The amount of this contraction from the temperature of the melted glass to that of the air could be determined with extreme accuracy for the two temperatures. In the large continuous working glass furnaces of my brother, the melting and working chambers are widely separated and have different but always approximately permanent temperatures. The temperatures are according to numerous determinations of my brother 1600° to 1700° in the melting chamber; and 1200° to 1300° in the working chamber. Two narrow-necked crucibles were prepared as similar as possible, and in each of the furnace chambers one of them was filled up as nearly as possible level with the horizontal rim of the crucible, with glass free from bubbles. Both were then removed from the furnace with the greatest care and placed in the annealing kiln. In order to prevent the upper surface of the glass from solidifying first, a cover specially prepared for this purpose and highly heated was placed over each crucible. It was found that after the cooling the glass in both crucibles had sunk equally. The amount of this sinking was afterwards ascertained in my laboratory by filling the space exactly with mercury, then the crucible was carefully broken and the volume of the mass of hard glass ascertained by weighing in water. A bubble of air which was found in the mass of the glass was determined by crushing the glass and brought into the calculation.

The result was :—

	Volume of the solid Glass cc.	Volume of the Cavity cc.	Proportion of the Volume in Percentage of the solid Glass.	Temperature.
I	1050	84.7	8.07	1650° C.
II	1080	36.4	3.37	1250° C.

The cubic expansion of liquid glass between the above temperatures amounted to 1.18% for 100° C., whilst a solid glass expands only 0.24 or about $\frac{1}{4}$ of this amount. This considerable diminu-

tion in the volume of the melted glass on cooling cannot be ascribed to the ordinary expansion of the body during heating. If it be assumed, that the cubic expansion of fluid glass with rise of temperature is considerably greater than that of solid, there is yet no analogy for so great an increase of the co-efficient of expansion with temperature. That the mass of glass does not further contract on final solidification from the still plastic condition was ascertained by blowing a wide glass bottle in a cold iron mould. A bottle taken out of the mould at a dull red heat had after cooling in the annealing oven a circumference of 293.3^{cm} . A lump of gypsum solidified in the same mould, which as is known takes place without shrinkage, had in the same part a circumference of 290.2^{cm} . If one assumes that the difference between the temperature of red hot and still plastic glass and that of the air amounted to 800°C ., and that the linear expansion of refractory bottle glass amounts to 0.0008 per 100°C . increase of temperature, the contraction of the hard glass would have been about twice as great as that here found, whence we may conclude that expansion takes place with actual solidification.

Still more decisive of the question whether there is a contraction or expansion of the silicates on solidification from the still plastic condition, is the collection which Mallet has made of measurements taken during a whole year, at the Plate Glass Company's works at Blackwall, of the size of plates of glass in the red hot viscous, and in the cooled condition. These gave a contraction of 0.53% . If one also assumes in this case a difference of temperature of 800°C ., and a co-efficient of linear expansion for solid plate glass of $\frac{1}{1166}$ or 0.0009 per 100°C ., the linear contraction of solid glass on cooling would amount to 0.72% , a small expansion would therefore have occurred on actual solidification. The question whether expansion or contraction occurs with silicates in passing into the crystalline condition has not yet been determined by experiment. It can hardly be decided by melting crystalline masses, for according to experience with such melting a great loss of weight takes place through volatilization. It is probable that the change of specific gravity on crystallization is as different with the different silicates as with other crystals, in which according to yet unknown laws, sometimes expansion and sometimes contraction takes place.

It follows from the above, that the assumption upon which

Thomson based his calculations, viz., that a diminution of volume of about 20% occurs, when a silicate passes from the fluid to the solid condition is not admissible. The contraction occurs when passing into the amorphous condition entirely, and when passing into the crystalline condition, at least certainly for by far the greater part during the conversion from the thin fluid to the viscous state. Thomson's calculation does not therefore prove as he assumes that the earth must have become solid in the interior by pressure, but that it must thereby have become either viscous or plastic.

This viscous condition, which quartz and silicates rich in quartz assume on cooling, and according to Thomson's calculations through pressure, makes it also explicable, that a solid crust could be formed out of heavier material, on the still fluid earth. When the cooling was so far advanced, that a solidification of the outermost layers of the earth ellipsoid became possible, they then passed into a viscous state, which would be favoured by the fact that water, carbonic acid, and other volatile bodies were evolved from it in the form of gas. These heavier layers must have sunk into the thin liquid magma, and thus have become according to Thomson's theory more viscous. There must therefore have been formed down to unknown depths a coherent viscous layer for the support and protection of the solid crust which next formed. This viscous plastic mass must on account of its mode of production have been riddled with layers and channels of lightly fluid magma, and so have abundantly allowed it access to the solidified crust and the surface of the earth.

The assumption that such a condition still exists, is opposed to Thomson's result, that the existing tides absolutely require a solid condition of the earth. In this respect I must however refer to a factor which, as it appears to me, has been left out of account by Thomson. This is the time which must elapse until the maximum of the deformation of the earth's ellipsoid limited by the attraction of the moon and sun has taken place. With the prodigious dimensions of the earth, this time must have been considerable, especially if one assumes the interior of the earth to be viscous, as is likely, even according to Thomson's calculations. How slowly viscous masses yield to a pressure impressed on them, is proved by a ball of pitch or other similar viscous substance, which only fully yields after months to the pressure caused by the

earth's attraction on it, and flows down into a cake. Even if the earth consisted of perfectly elastic material, the liquid ellipsoid could only be fully completed after the lapse of a certain time, as also follows from the consideration, that sound in water would occupy two hours to pass from the centre of the earth to its periphery. Viscous masses transmit sound only in a very slight degree. It therefore appears likely that the earth tide (even if it be assumed that no considerable rigidity or elasticity is to be ascribed to the crust) remains so far behind the sea-tide with the rotation of the earth, that it can only exert a slight reducing influence upon it.

With the view adopted by Thomson, that the earth had already become solid on the first formation of its outer surface, and that the volcanic outpourings of lava arose from hollows in the solid interior of the earth, in which non-solidified masses had remained, it is not comprehensible by what forces the lavas are raised to the level of the mouths of the craters. If it is also assumed that the enclosed lava, remained still unsolidified on the progressive cooling of the earth, because it consisted of silicates more easily fusible than the surrounding solid masses, yet it must always have become colder, and its volume must therefore have diminished in a higher measure than that of the hollows in which it was. If these are in communication with the atmosphere through the channels of the crater, no lava could have been driven out by progressive cooling, but on the contrary, air must have been drawn in. The entrance of rain-water could not have produced any upheaval of the lava, as it could either escape through the passage of the crater in the form of vapour, or the further entrance of water through the existing pressure of the vapour must have been prevented. Even more difficult of explanation according to Thomson's theory would be the formation of the several thousand feet of aqueous strata, which almost without exception cover the whole of the earth's surface. If the sea at first covered the whole earth, and on account of its high temperature must have exerted a much greater dissolving and destroying influence on its rocky substratum, this action could only have reached to a slight depth, as the strata deposited from the sea must have soon protected the primary rocks from further destruction. But it is quite inconceivable in what way there could have arisen the stratified deposits

of variable composition, often amounting to many thousands, and uniformly covering wide stretches of land. Geologists formerly explained these strata, though inconclusively, by frequently repeated risings and sinkings, by which a continuous change between dry land and sea bottom had taken place. But quite apart from the question, by what force these risings and sinkings so frequently repeated could have been brought about, and why no portion of the earth's surface had been forgotten in this see-saw, the thickness of the sedimentary strata is not explained by this hypothesis. For even if a sedimentary stratum was formed of sufficient thickness to protect the primary rock lying under it from further disintegration, at the next upheavals these deposits were again destroyed by rain-water and reconveyed to the sea. A further considerable increase of the sedimentary strata could therefore no longer take place. To explain their formation and especially their stratification, their origin must necessarily be sought in the interior of the earth. If after the formation and gradual cooling of the crust, the sea were still combined for the most part with the magma, as was formerly considered probable, then watery magma must have been conducted through innumerable craters to the shallow sea still covering the whole earth, the dissolved or soluble substance of which the sea-water took up, to diffuse it by its currents, and then to employ it for the production of sedimentary strata. Only when the sediment had for the greater part been deposited, did the continents begin to be heaved up, and further transformations of the portion of the now dry upper surface could now take place through the influence of rain water, and further deposits kept up by organic life on the sea bottom. As crystalline deposits must also have been separated out of the gradually cooling magma on the inner surface of the crust of the earth, the water contained in it would be so much the poorer in solid and dissolved substances the thicker the earth's crust had become, and the more slowly cooling took place. On this ground it is likely that the period of the fusible mud volcanoes was followed by another period of hot springs, which continually warmed the sea, and thereby made organic life possible in the highest latitudes. As finally also these springs dried up with a few feeble exceptions, and sea and atmosphere had gradually cooled down in the higher latitudes, the warmer sea in the lower latitudes must have produced by its greater

vaporization strong precipitates which forced down its temperature and called forth the glacial period.* Only after the heating influence of the interior of the earth had almost entirely disappeared could the present climatic influences arise.

The period of the separation of the sedimentary masses through eruption of magma containing water and carbonic acid must have been accompanied by an increased diminution of the volume of the fluid or plastic interior of the earth. Mallet had already shown that this diminution of volume which he ascribes only to the cooling by heat conduction of the still thin crust of the earth, and not simultaneously to the loss of substance of the interior of the earth by separation of the greatest part of the sea and of the sedimentary masses, must have forced the crust of the earth by folds, raising of mountains, and crushing of rocks in its weakest points, to again join the plastic nucleus. In fact one can only explain these geological phenomena with Dana and Mallet by the action of forces working tangentially in the crust of the earth. The solidity of the crust of the earth and its reaction against the fluid or at any rate plastic interior is very slight even under the most favourable assumption, considering the great diameter of the earth. Assuming that it consists of a homogeneous mass of basalt without cracks or weakened places of 100 kilometres in thickness, and that the absolute as well as the reacting strength of the basalt is 2000 kilogrammes per square centimetre, then the pressure this envelope exerts on the nucleus is about 30,000 atmospheres. If it be further assumed for the sake of simplicity that the earth is a ball with a circumference of 40 million metres, then a great section through the solid part of the hollow ball would give a cross section of $4 \times 10^7 \times 10^5 \times 10^4$ or of 4×10^{16} square centimetres. The absolute strength of the cross section would amount to 8×10^{19} kilogrammes. To burst the ball, there must be a pressure in the interior, exerting a greater pressure on the cross-section of the whole ball. As one atmosphere exerts a pressure of about one

* Dove has already pointed out that greater precipitates reduced the temperature of the latitudes near the polar regions, to account for the greater extent of the ice region of the southern hemisphere. As through the increased precipitate in the atmosphere more latent heat becomes free, it will bring about an extension of the cold zone at the expense of the cold of the higher latitudes. The polar temperature must have been higher in the glacial period than now.

kilogramme on each square centimetre, and as the cross-section of the whole earth is $\frac{(4 \times 10^7)^2 \times 10^4}{4\pi} = \frac{4 \times 10^{18}}{\pi}$ square centimetres,

the number of atmospheres for equilibrium is $\frac{8}{4} \times \frac{10^{19}}{10^{18}} \pi = 20\pi$ or

62·8. An increased pressure of 63 atmospheres, or the pressure of a column of molten rock of about 250 metres, would therefore suffice to burst the solid covering of the earth on the supposition made, and an equal reduction of the opposed pressure of the fluid mass would suffice, to toss it together, or to crush it at its weakest points, or to heave it up, in the line of least resistance in the form of enormous sheets as continuous chains of mountains.* Mallet now assumes that this crushing of the crust of the earth in con-

* The above assumption that the resistance to pressure is equal to the absolute resistance, is evidently not exactly correct. The first must probably be assumed to be considerably greater, but quite inconsiderable compared with the immense tangential forces which make their appearance by partial removal of the counter pressure of the fluid interior of the earth in the earth crust.

Resilience, or the resistance which bodies oppose to crushing stress, still forms an obscure branch of mechanics. The nature of the active resisting forces is neither clearly defined, nor do absolute experiments exist applied according to the same method to the same material, from which a proportion or dependence between the absolute rigidity and resistance to pressure can be deduced. Existing experiments on the resistance to pressure of stone have been in part very incorrectly made. For instance, in the geological textbook of Pfaff* experiments are quoted which show a quite excessive resistance to pressure for stone. Chalk should, according to it, bear a pressure of 21,800 atmospheres. The fault lay in the stone to be crushed having a much greater surface than the compressing stamp, so that the force required to burst apart the surrounding uncompressed material was not taken into account.

As is well known, elastic bodies offer an equal resistance within their limits of elasticity to a small expansion and compression. This makes it likely, that a more real difference does not exist, where the limits of elasticity are exceeded, where the body is therefore split or crushed. One can now set the problem of determining the load, which a cylinder most favourably loaded in the direction of its axis is able to bear; when only the force, with which the pieces hold together, therefore the absolute rigidity is really to be considered as acting.

Let A B be a very thin, cylindrical disc of hard, homogeneous and elastic material, which lies without friction on a fixed level plane, and α the coefficient of absolute rigidity of the layer. If a concentric ring of internal radius x , and external radius $x + dx$ is uniformly loaded, it will be compressed, and exert a side pressure inwards and outwards corresponding to the load. The former will tear the surrounding ring asunder if the pressure on the plane of section of the whole ring is greater than the absolute strength of the wall of the ring. Then as the thickness of the ring does not enter into

* Universal Geology as an exact Science, by D. F. Pfaff, p. 302.

sequence of the cooling and contraction of its nucleus has brought about the present formation of the earth's surface not only at

the calculation, there would be equilibrium if z represents the pressure requisite for equilibrium on the unit surface of the ring.

$$\begin{aligned}\text{When} \quad z \times x &= (r-x) a \\ z &= \frac{(r-x) a}{x}\end{aligned}$$

The pressure dP bearing on the surface of the ring $2\pi x \times dx$ is if P represents the total pressure

$$dP = \frac{(r-x) a}{x} 2\pi x \cdot dx = 2a\pi (r-x) dx;$$

the integral, taken between the limits r and 0 gives

$$P = ar^2\pi.$$

The crushing of such a disc, supported equally without friction, would therefore require with regular loading exactly as much force as its tearing. With a uniform loading of the surface, the outer ring would break earlier, its resistance to pressure would therefore be less. A more correct expression for the resistance to pressure would, according to this calculation, only be obtained if the side pressure which a pressed piece exerts is equal to the pressure itself as in fluids, which is however not the case. As, however, the side pressure is less, and dependent on the nature of the material, the resistance to pressure must be greater than given by the above calculation. But the calculation shows that the resistance to pressure is dependent on the distribution of pressure on the upper surface of the compressed body, and explains why experiments on resistance to pressure gave such dissimilar results.

The resistance to compression is very much modified by the formation of vaults in certain cases. A hollow ball with equally thick homogeneous walls may be assumed to be a perfect arch. If a uniform external pressure is applied to it, it will be compressed correspondingly to the pressure without breaking. This follows from the consideration that a giving way of the molecules of the shell of the ball towards the outside cannot occur, as they will be uniformly held back in their places everywhere by the external pressure. Neither can a giving way take place towards the interior, as with this, on account of the concavity of the inner surface, a nearer approach of the molecules to one another, and therefore a greater local compression occurs than corresponds to the external pressure. The result of the external pressure can therefore only be a uniform reduction of the diameter of the hollow ball dependent on the pressure. If, on the other hand, the spherical shell is compressed by internal instead of external forces, these considerations do not hold good for the outer surfaces. In this case an unhindered giving way of the molecules can take place through the resulting tangential forces. The throwing up of portions of the crust of the earth by such preponderating tangential pressure must therefore have taken place externally and not internally. It hence follows that channels can reach down to the lowest depths in the mass of rock without being compressed. It is not, however, necessary that they should be circular in section, because the surfaces of greatest resistance or the arch surfaces are spontaneously formed in the surrounding rock wall. It further follows that there can be no question of an arch formation of the greater part of the solid crust of the earth, through which, according to the opinion of many geologists, the formation of great hollow spaces beneath it is best explained. The very basis for arch formation is absent, a uniform pressure working on the outer surface.

earlier periods, when the cooling took place more rapidly on account of the slight thickness of the crust, but also that this activity still continues and that the work performed in the pressing together, crushing or displacing of the rocks in the line of least strength combined with friction is transformed into heat and causes a local fusion of the rock, the products of which then partly appear as an outflow of lava from volcanoes. Roth has already brought forward, that these displacements and crushings must have taken place slowly at great intervals of time, and hence could not have produced the heat necessary for melting the rock. It would moreover be impossible to explain the great quantities of gas and water which the volcanoes discharge by means of such local heatings and fusions of the rock.

But if now, as well on mechanical as geological grounds, the opinion of a complete solidification of the body of the earth must be rejected, and altered to the view that the interior of the earth still fusible, or at least still in the plastic condition, is surrounded by a hard crust of considerable thickness, it may be asked what forces heaved up the eruptive rocks in earlier periods, and at the present day threw up the lava to the mouths of high lying craters? With the slight force of resistance of the hard crust which has been referred to, the idea of an over pressure of the fluid interior must be altogether abandoned for there is no cause, with the continually proceeding cooling, which could bring about such a pressure, and besides the least over pressure would be at once equalized, through yielding of the crust so little able to oppose internal pressure. But it further follows, that the crust is everywhere, at least at all great sections supported by the underlying fluid or plastic mass, that accordingly hydrostatic equilibrium must exist everywhere in the earth. Now the easily fusible, alkaline, and water-holding lavas, which have remained in comparatively narrow channels and hollow spaces, between the viscous aggregated silicate masses, and forming the basis of the hard crust, must have a less specific gravity than the earth's crust and the viscous silicate masses. If, therefore, a channel is opened through fissures in the just solidified lower layers of the hard covering to the still existing old channels leading to the upper surface, the lava in them must rise up until the hydrostatic pressure is balanced, or the channel is again stopped up by the

viscous matter pressing onward. This forcing up of the fluid matter through hydrostatic pressure will be considerably augmented in the higher lying portions of the crater by vapour and gas which are released from the lava by the reduced pressure. A more difficult question to answer still remains, how new eruptive activity can occur, if the opening of the crater is filled with cooled lava from the preceding eruption? The matter is not alone to be explained by newly arising fissures, which open new channels from the earth's interior to the crater channel, although they assist to a certain extent. A melting heat is certainly necessary in order to open the old channel filled with solid lava, which cannot be derived from the melting lava freshly coming up from below, for this itself would soon be thereby consolidated. A fairly satisfactory explanation of the phenomenon that the old lava channels again open, may, however, be found in the fact that the lava by its hardening from the then fluid condition shrinks by about one-tenth of its volume, and that the viscous condition it thereby takes hinders it from sinking again in the crater channels. It will, therefore, frequently split in hardening, and permit the passage after hardening of combustible gases and heated steam, which press out anew from within. These, partly by the giving up of their own heat, and partly by the heat generated by combustion of the gas with air drawn in from above or sideways, will cause the fusion again of lava remaining behind from earlier outbreaks, and thereby bring about another period of eruption.

The first inducement to a new eruption will, as already mentioned, be caused by ever newly arising cracks in the deep layers of the crust in the neighbourhood of the crater. The circumstance that outbreaks of dormant volcanoes are almost always announced by earthquakes is further evidence. That it is exactly in the neighbourhood of volcanoes, be they active or extinct, that frequent earthquakes occur, does not, however, prove that earthquakes are the consequence of volcanic activity; the opposite may probably be assumed, that districts where earthquakes frequently arise favour volcanic activity. There can be no doubt that frequent rents must take place in the later rock formations of the interior crust. These deposits will vary in nature according to the local condition of the magma from which they are separated.

Their co-efficient of contraction must therefore also vary. Local tensions must therefore occur as the cooling of these lower layers proceeds, which must lead to rents in masses which shrink more than others enclosing them. This cracking is further promoted by these later rocks being firmly connected with the older crust, which, already split into chinks at previous periods, is now subject to smaller differences of temperature. As of two glass plates melted together, that one must be torn asunder which is most contracted on cooling, so the more recent rocks must crack on cooling. The formation of such rents, perhaps extending over great distances, and forming wide gaps, must necessarily be perceived as earthquakes on the surface, which must be so much the stronger the older is the split rock, the nearer therefore the ruptures approach the surface. Ruptures from the same causes can also arise under certain circumstances in the upper sedimentary strata of rock. A rupture once produced will, as a rule, have many others in its train, until elastic equilibrium is restored and the fissures are again filled up by the neighbouring deeper lying layers under the existing pressure.

It is to be assumed that such ruptures are specially promoted in volcanic districts exposed to frequent earthquakes through the nature of the rocks and of the surrounding rock masses. Frequently local dislocations of the earth's crust brought about by tangential forces in consequence of continual reduction of the volume of the fluid nucleus may be the cause of the tearing of the layers of rock.

The cause of volcanoes may hence be considered as being in strata of rocks already much split, and disposed to further cleavage, into which extends the crater passage, probably also running into numerous ramifications. The older fissures are filled with viscous magma pressed in and difficult to melt, subsequently hardened by cooling. Owing to newly-formed fissures which again cause others, fractures may arise, which by the formation of dome-like arches would be sheltered from the pressure of the masses of rock lying above. The plastic magma freed from pressure penetrates into these, whilst the thinly fluid lava enclosed in it as well as the freed vapours and gases, rush on in front. If at the same time a communication with the crater passages leading to the upper strata is set up through the new

fissures, the latter commence their heating and melting activity, whilst they escape through the fissures of the older filling of the crater passage, until at last the lava collected over the plastic magma is driven up into crater passage again opened, through the hydrostatic pressure which is exerted on it; whether it reaches the mouth, and appears to view, depends on the quantity of thinly fluid lava collected, also on the height of the volcano and the specific gravity, and amount of gas and water in the lava. Some of the very lofty volcanoes eject no more lava, but only throw out strong flames and water. In them the hydrostatic equilibrium is already restored by the existing condition of the lava, before the column of lava arrives at the top of the crater. This may be the reason why still active volcanoes lie for the most part in or near the sea.

But if the mechanism of volcanic activity, and many other geological circumstances, may be explained in a fairly satisfactory way by the assumption of a solid earth crust floating on a fused or plastic mass, a fact yet remains which can only be brought into harmony with this hypothesis by the introduction of an altogether new assumption. This is the considerable elevation of continents above the sea bottom, and the still existing secular rising of many stretches of land. The height of the table land of Asia above the bottom of the Pacific Ocean can be estimated at least at 12,000 metres, and if the weight of sea water reduced to that of rock is taken into account may be estimated at 10,000 metres. This represents a difference of pressure of about 1,000 atmospheres. It appears inevitable, with the slight strength of the crust of the earth referred to, that the table land of Asia and the surrounding continent must have sunk to the position of equilibrium, and the bottom of the sea must itself have been raised. If the assumption of a fused interior of the earth cannot be abandoned, it must be assumed that the necessary hydrostatic balance is furnished by the difference in the specific gravity of the rock of which the continent and sea bottom is formed, that the sea bottom is therefore of heavier rock than the continents, or that the half fluid mass found under the solid crust has such a thickness and such a different specific gravity that the difference of pressure is equalized. The secular rising would then be the local consequence of this difference.

ON THE DEPENDENCE OF THE ELECTRIC CONDUCTIVITY OF CARBON ON TEMPERATURE.*

MATTHIESSEN † first drew attention to the remarkable property of carbon of conducting electricity better at high than at low temperatures. For the best conducting and at the same time hardest and heaviest form, viz., gas retort carbon, which is produced by the disintegration of overheated illuminating gas, and is deposited on the walls of the retorts of gasworks, he found the specific conductivity 0.0236 (mercury being the unit) at 25° C., and between 0° and 140° a diminution of the resistance by 0.00245 per degree Centigrade.

Beetz found the fact of the increase of conductivity with rising temperature to obtain only for so-called artificial carbon which is moulded from coal-dust with a small binding addition of tar or syrup, and then heated, whereby the sugar is separated into escaping gas and carbon, but not for carbon rods cut from retort carbon. With these he was unable to observe any increase of conductivity with increase of temperature. The increase in conductivity of so-called artificial carbon, Beetz explained by more powerful pressure which the loosely aggregated particles must exert on each other when expanded by heating. I have often myself had the opportunity of proving by means of experiments made elsewhere that Matthiessen's statement was correct. All the more remarkable therefore to me was the result of a recent work of Felix Auerbach, brought by Riecke before the Royal Society of Science of Göttingen in January, 1879, wherein it is stated that gas retort carbon behaved as regards its electric conductivity like metallic alloys, as its resistance increased with ascending temperature in an increasing proportion. That so exact an experimenter as Matthiessen could have been so completely at fault I could hardly believe, although Beetz also could observe no increase in the conductivity of gas retort carbon. On the other hand, Auerbach's experiments were evidently made with care and with good instruments. Unfortunately none of the three observers had described

* Monthly Report of the Berlin Academy of Sciences of 5th January, 1880

† Pogg. Ann. Vol. CIII., p. 428.

their experiments with sufficient detail to enable one to discover by critical examination the cause of the difference in their results. In the general arrangement of Auerbach's experiments, the only essential points to be criticised were the way in which the carbon rods were heated and their small resistance. The uniform heating to a determined temperature of rods 122^{mm} long and 6^{mm} thick could only be carried out with difficulty in a chamber full of air. From the description of the experiment it cannot be found out how the heating of the air was brought about. The supposition that the temperature of the rod agreed with that of the thermometer, when no further change of resistance was to be observed on the galvanometer is hardly permissible for exact measurements. As only the means of several measurements at each temperature are given without specifying the deviations of the separate measurements from one another, the correctness of the temperatures given for the carbon rods cannot be controlled. Anyhow the agreement of the observed and calculated results is great enough to exclude the idea that the final result of Mr. Auerbach's measurement could only depend upon errors of observation. As an unequivocal determination of the question, whether and to what extent the resistance of carbon increases or diminishes with variations of temperature is not only of great scientific interest, but also possesses great technical importance, I determined on a complete examination of it.

I had prepared cylindrical carbon rods of various lengths and diameters. These were electrolytically coppered at the ends for a distance of 15^{mm}. The wires of a copper strand were laid on to the coppered ends and bound round a few times with fine copper wire to fasten it to the carbon. The carbon end so prepared was then put back into the copper solution, and so much copper deposited on it that the copper wires were cemented firmly to the first copper covering, and therefore also to the carbon. The heating of the carbon prepared in this way was carried out in a bath of non-conducting liquid. For low temperatures up to 60° C. I used heavy petroleum; for temperatures up to 270° C. melted paraffin. The liquid was placed in a trough of tin-plate, and could be heated by means of a burner placed below it, or cooled by placing the trough in snow. The trough, about 260^{mm} long, 75^{mm} broad, and 80^{mm} high, was covered with a slate slab, in which

were fitted two copper bolts with suitable terminals attached at both ends. Into the lower terminals the copper ends of the carbon were fixed, and for greater safety soldered to them. By means of the upper terminals of the slate cover of the trough, the carbon was connected up to a bridge arrangement, which consisted of two exactly adjusted resistances in the ratio of 1 to 100, and a resistance box by means of which resistances from $\frac{1}{10}$ th to 10,000 units could be inserted. A very sensitive mirror galvanometer was employed with four coils and a pair of astatic needles. To control the arrangement and judge of its sensitiveness, as well as of the exactness of the measurements, a second resistance box was next inserted in place of the carbon; and it was proved that when balance was obtained, the resistances of the two wires were always in the ratio of 1 to 100, when the resistance of the leading wires which was 0.033 of a unit was taken into account. The insertion of $\frac{1}{10}$ th of a unit in the large branch of the bridge, above or below the balance, produced a deflection of the mirror of about 20 divisions of the scale, when 1 unit was inserted in the smaller branch of the bridge. The temperature of the bath was read off by means of two accordant Feuss thermometers, of which one was arranged to read temperatures from -30° to $+70^{\circ}$, with divisions of 0.1 degree, and the other temperatures from 10° to 300° , with divisions of 1° . The thermometer was introduced into the bath by means of a slot in the side of the slate cover, which allowed of its being moved to and fro in the neighbourhood of the carbon rod throughout the whole length of the bath; so as to bring about its uniform temperature, and the correspondence of the temperature of the thermometer and carbon. It was easy for me in this way to produce any desired temperature, and to maintain it until my son Wilhelm who assisted me in these experiments had finished plugging the comparison resistances. It was usual to measure temperatures from 0° to 250° with the same piece of carbon one or more times in an ascending and descending cycle.

The resistance of the leading wires amounted in all the measurements to 0.033 unit, and is subtracted in Column 4 from the resistance read off in Column 3.

In Column 8 the percentage increase of the conductivity between two consecutive measurements is calculated for each degree of temperature. The measurements of the same carbon differ con-

No. of the Carbon.	Temperature.	Observed Resistance.	Resistance Connection.	Temperature Difference.	Resistance Difference.	Do. for 1° C.	Co-efficient.
1	270	Units. 1·223	Units. 1·190	.	Units.	Units.	
	260	1·226	1·193	10	- 0·003	- 0·00030	0·00025
	240	1·233	1·200	20	- 0·007	- 0·00035	0·00029
	220	1·242	1·209	20	- 0·009	- 0·00045	0·00038
	200	1·249	1·216	20	- 0·007	- 0·00035	0·00029
	180	1·255	1·222	20	- 0·006	- 0·00030	0·00025
	160	1·259	1·226	20	- 0·004	- 0·00020	0·00016
	140	1·267	1·234	20	- 0·008	- 0·00040	0·00033
	120	1·273	1·240	20	- 0·006	- 0·00030	0·00024
	100	1·283	1·250	20	- 0·010	- 0·00050	0·00040
	80	1·292	1·259	20	- 0·009	- 0·00045	0·00036
	60	1·313	1·280	20	- 0·021	- 0·00105	0·00087
	40	1·321	1·288	20	- 0·008	- 0·00040	0·00031
	25	1·326	1·293	15	- 0·005	- 0·00033	0·00026
	3	1·333	1·300	22	- 0·007	- 0·00032	0·00025
Mean Coefficient = 0·000331.							

siderably from one another on different days, which is partly explained by variations in the temperature of the room, whereby the ratio of the resistances of the branches of the bridge was somewhat altered. Exact experiments at temperatures higher than 270° (which can still be obtained with a paraffin bath) are difficult to carry out, as sure methods of heating and exactly determining the temperature of the carbon are wanting. In order however to satisfy myself as to whether the resistance of the carbon diminished regularly up to red-heat, I had a copper tube prepared about 200^{mm} long and about 20^{mm} internal diameter. The carbon rod was kept suspended pretty well in the middle of the copper tube, by fixing its coppered ends into two pierced gypsum stoppers. The copper tube thus arranged was laid in a small Chamotte furnace, and heated by means of a charcoal fire burning uniformly in it. The resistance of the carbon at the temperature of the air before heating was 1·452 unit. The resistance continually diminished during heating. When the copper tube was heated to such a temperature that small pieces of tin placed upon its surface melted, the resistance was about 1·375 unit, and when zinc melted it was 1·298 unit. If the fusing point of tin is taken at 230° C., and that of zinc at 423° C., then the temperature of the room being 20° C., this gives the percentage increase of conductivity between the temperature of the room and that of fusion of

tin as 0.00025, and between this and the fusing point of zinc as 0.00029 for each degree of temperature. Probably the carbon had not fully attained the temperature of the tube. The heating was afterwards continued to a dark red-heat of the copper tube. The resistance of the carbon changed very irregularly and uncertainly. When the temperature of the tube was maintained for some minutes at a red-heat, it became constant at 1.300 unit. The carbon was then quickly removed from the furnace, and the tube quickly cooled. In this way the resistance of the carbon steadily increased until when the tube had again returned to the temperature of the room, it remained steadily at 1.685. The important increase of resistance observed after the carbon had cooled compared with that at the commencement of the experiment may be ascribed to the circumstance that the oxygen remaining in the tube consumed a portion of the carbon, and had thereby permanently increased its resistance. The increase of resistance during the slow heating from the temperature of melting zinc to red-heat points to the same conclusion. During the rapid cooling from this temperature to that of the room no further combustion of carbon could occur. If red-heat is taken at 900 C., the increase of resistance during cooling gives a percentage diminution of the conductivity of 0.00033 per degree, an agreement with the values found for lower temperatures, which considering the uncertainty of the temperature taken is only accidental. From this value it may however be considered as proved that the conductivity of carbon increases up to red-heat.

The circumstance that I, like Matthiessen, connected the ends of the carbon with the connecting wires by an electrolytic deposit of copper, whilst Auerbach effected it by dipping the ends of the carbon into melted solder, and allowing them to cool in it, made it appear probable to me, that in this is to be sought the chief cause of the incorrect results of the latter's experiment. I have already in 1860 * referred to the observation, that metal wires, when dipped into a mercury bath without being previously amalgamated, showed a surface resistance which was evidently due to a badly conducting air-film condensed by molecular attraction, on the outer surface of the metal through which the current had to pass.

* Pogg. Ann. Vol. CX., p. 11.

As the carbon rods which Auerbach used were comparatively thick for their short lengths (about 16^{mm} square), and had consequently only a low resistance, the resistance of such a film of air must have exercised a considerable influence on the results of his measurement, as it must be much stronger on the surface of the carbon than on metals, on account of the greater occluding power of the former. In proof of this conjecture I broke a carbon rod, which had already served for measurements, and showed a decided increase of conductivity with increasing temperature, 20^{mm} from the coppered end, and dipped the free end according to Auerbach's method into melted solder, to which after cooling the other leading wire to the bridge was firmly soldered. The result was a surprising one. The resistance of the carbon rod now about 10^{mm} long increased markedly with rising temperature. Another experiment with a longer carbon rod of which one end again was connected with the bridge wires by means of solder according to Auerbach's method, gave increased conductivity with rising temperature, but its coefficient became much smaller. An exact measurement proved impracticable, as the resistance especially with high temperatures was too fluctuating.

Lastly, a rod of gas retort carbon of square section, 120^{mm} in length and 63 sq.^{mm} in section had solder poured round its ends, and then the resistance was measured at different temperatures. The measurements were very irregular, yet a decided increase of resistance with rising temperature was to be observed. Then the solder caps were removed and the ends electrolytically coppered. Just as decided and regular a reduction of the resistance with rising temperature was now observed.

By means of this experiment it is quite certain that by Auerbach's method of pouring solder round the ends of the carbon no direct union of the carbon with the metal is obtained, that on the contrary (just as by the immersion of a non-amalgamated metal in mercury), a film of condensed air separates the carbon from the surrounding metal after the cooling of the solder, and that Auerbach's varying results are thus completely explained.

This however does not yet decide the question whether the air-film increasing the resistance itself possesses the property of increasing its resistance with increasing temperature in the observed proportion. It may also be thought that the unequal expansions

of the metal and of the carbon brings about a loosening and contraction, and reduction in the number of the actual points of contact between the carbon and the metal. That with electrolytic coppering a separating film of air does not occur is certainly to be ascribed to the circumstance that the liquid dissolves the gas condensed on the upper surface of the carbon before the precipitation of the copper begins. On this ground it is also to be recommended to boil the ends of the carbon before beginning the coppering, or to allow it to stand for a short time in the heated coppering liquid. In place of coppering I have gilded the ends of the carbons with good results, in a hot solution of cyanide of gold. The copper leading wires were then metallicity connected in the manner described with the gilded surface by means of a copper deposit.

The following series of experiments was made with a round rod of carbon 2.43^{mm} thick, and 148^{mm} in length between the terminals, which was cut from a selected piece of very dense and fine grained Berlin gas retort carbon. In this as well as in the later series of experiments not only was the resistance more exactly measured, but the temperature was kept constant for a longer time than in the earlier experiments. (See Table A on the following page.)

The specific conductivity of gas retort carbon is according to this 0.0136 at 0° C. (mercury = 1) and the coefficient of increase of conductivity 0.000345 per degree Centigrade.

The so-called artificial carbon which is now specially used for the production of the electric light, is as a rule moulded from powdered gas retort coke combined with tar or concentrated syrup as the cementing material, and made hard and well conducting by repeated baking and soaking. Beetz found a considerable increase of conductivity with increasing temperature with this material, whilst he did not observe it with rods from retort carbon. It does not appear unlikely that the carbon from the decomposed tar or sugar which separates the particles of gas-coke, possesses other properties than gas retort coke, as the carbon reduced from solid hydrocarbons retains hydrogen persistently even when highly heated, and then becomes a very bad conductor, like for instance charcoal which has not been heated hard enough and long enough. Such a badly conducting intervening layer could also specially affect the coefficient of increase of the conductivity. Experiment however has not yet made this clear. Two different

artificial round carbon rods made in France were fitted with leading wires in the manner described, and their resistance measured at different temperatures, with the results given in the following table. (See Table B below.)

TABLE A.

Berlin Gas Retort Carbon.						Remarks.
Resistance.		Temperature.	Difference of Resistance.	Difference of Temperature.	Coefficient of Increase per Degree.	
Measured.	Actual.					
2-2443	2-2095	75				} The carbon was used for the first time.
2-2260	2-1912	100	0-0183	25	-0-000320	
2-2070	2-1722	125	0-0190	25	-0-000364	
2-1864	2-1516	151	0-0206	26	-0-000347	
2-1659	2-1311	175	0-0205	24	-0-000397	
2-1660	2-1312	173-5	0-0156	22-5	-0-000323	} Measured on the following day.
2-1816	2-1468	151	0-0184	24-5	-0-000346	
2-2000	2-1652	126-5	0-0192	25-3	-0-000347	
2-2192	2-1844	101-2	0-0193	25-2	-0-000347	
2-2385	2-2087	76				
2-2385	2-2087	76				
2-2196	2-1848	101	0-0189	25	-0-000343	
2-2028	2-1680	125	0-0168	24	-0-000320	
2-1857	2-1509	149-5	0-0171	24-5	-0-000323	
2-1674	2-1326	145	0-0188	25-5	-0-000334	
2-1492	2-1144	201-5	0-0182	26-5	-0-000322	

TABLE B.

	Measured Resistance.	Carbon Resistance.	Temperature.	Difference of		Coefficient.	Remarks.
				Resistance.	Temperature.		
Artificial	1·4091	1·3850	230	0·0142	30	—0·000338	The Resistance of Connections, 0·0241.
Carbon No. 1	1·4238	1·3902	200	0·0211	50	—0·000298	
Length,	1·4444	1·4208	150	0·0209	50	—0·000288	
148 mm.	1·4658	1·4412	100	0·0231	50	—0·000317	
Area,	1·4884	1·4643	50	0·0118	24·6	—0·000329	
4·8208 sq. mm.	1·5002	1·4761	25·4				
Mean . . . —0·000314							
Artificial	1·5085	1·4692	75				The Resistance of Connections, 0·0343.
Carbon No. 2.	1·4948	1·4595	100·5	0·0097	25·5	0·000261	
Length,	1·4830	1·4487	125	0·0108	24·5	0·000302	
155 mm.	1·4712	1·4369	150	0·0118	25	0·000326	
Area,	1·4598	1·4255	176	0·0114	26	0·000306	
4·465 sq. mm.	1·4500	1·4157	199	0·0098	23	0·000299	
	1·4506	1·4163	190	0·0099	24	0·000289	
	1·4605	1·4282	175	0·0100	25	0·000278	
	1·4705	1·4369	150	0·0110	25	0·000320	
	1·4821	1·4478	125	0·0114	25	0·000312	
	1·4935	1·4592	100	0·0114	24·8	0·000312	
Mean . . . 0·000301							

It consequently follows that the artificial carbon rods made by pressure out of coke powder, like those cut out of gas retort carbon, show a great increase of conductivity with increasing temperature, and that the increase is not quite so great as with gas retort carbon.

The different results obtained by other observers are probably also to be attributed entirely to defective connection of the ends.

In the experiments described no decided increase or diminution of the coefficient of increase is apparent. I hesitate all the more to express any decided opinion in this respect from the measurements given; as they have not given in general such decided and certain results as the method employed led me to expect. It must be left to a closer investigation to decide whether these irregularities, hitherto unaccountable, are to be sought in that the conductive connection is not to be considered perfect even with electrolytic coppering, or in that carbon is affected by similar influences as alter the conductivity of selenium. The explanation which Beetz has given for the phenomenon of the increase of conductivity of carbon with increasing temperature, could only be applicable to carbon powder or loosely aggregated carbon, enclosed by solid walls expanding less than the carbon. As the whole volume of a body expands in the same proportion as that of its parts, an increased pressure of the parts with uniform increase of temperature cannot occur with non-enclosed bodies. Beetz in support of his hypothesis appealed to some experiments he had made with metal filings. Their resistance diminished both with external compression and heating. That this must occur, if a compression of the powder actually takes place there can be no doubt, and experiment fully confirms it. If the powder were partially enclosed by the walls of the vessel, a reduction of the resistance might very easily occur. The air condensed on the outer surface of the particles of the powder may very likely have had some influence. The conclusion from the powder cannot however be carried back to coherent masses without enclosing walls, like moulded carbon. A simple experiment shows that even a great pressure does not alter the conductivity of moulded carbon. If the ends of a cylinder of carbon are provided by means of electrolytic coppering with reliable soldered connections, and the carbon rod is exposed to powerful pressure in the direction of its axis, the resistance does not change in the least,

even when the pressure is increased till the carbon is ruptured. This shows that well impregnated and baked moulded carbon is to be looked upon as a hard if still porous body, and not as loosely aggregated moveable powder. This holds good in a much higher degree with unpowdered solid gas retort carbon. The production of this carbon proceeds in a similar way to the electrolytic separation of metals, viz., as we have already stated, the carbon in immediate contact with the surfaces of the retort walls becomes free, and at the moment of becoming free joins together by molecular attraction. Gas retort carbon is hence not to be looked upon as powder baked together, but as a solid mass of carbon. That the specific weight of gas retort carbon is different is rather to be ascribed to the inclusion of little hollow spaces, and foreign solid bodies than to a difference in the mass itself. The general property of carbon of conducting better at high temperatures must hence be looked upon as a property of the material itself of the carbon and not as a consequence of its structure.

Analogous to this behaviour of carbon is that of electrolytes, among which, according to Hettorf, copper sulphide and other solid compound bodies are to be included—and of the simple bodies selenium and tellurium. The latter becomes non-conductive when quickly cooled from the molten condition as does the diamond. If it is heated to 100°C. , it becomes crystalline and then conducts electricity like carbon, in such a way that its conductivity increases with increasing temperature. Selenium loses latent heat by heating to 100°C. ; it is hence probable that this disengagement of latent heat has made it a conductor of electricity. If quickly solidified so-called amorphous selenium is heated near to its melting point, *i.e.*, to over 200°C. , and maintained for some time at this temperature, it loses still more latent heat, and then becomes, as I have shown,* much more conductive. It now conducts electricity like a metal, that is its conductivity diminishes with increase of temperature. It hence appears likely that the property of crystalline selenium still retaining latent heat, of conducting electricity like the electrolytes and carbon, viz., that its conductivity increases with temperature, depends on its retaining latent heat. As latent, like free heat, causes a

* Pogg. Ann. Vol. CLIX., p. 127.

hindrance to electric conduction, or is very likely the cause of electric resistance, and as the stability of allotropic conditions which hold latent heat, diminishes or is quite lost through heating, when the latent heat disappears, the obstacle which the latter opposes to the passage of the electric current becomes less with increase of temperature. The better conductivity of carbon at high temperatures can hence be explained as in the case of crystalline selenium by assuming that like it carbon is an allotropic modification, containing latent heat, of a hypothetical metallic carbon.

There is also in favour of this assumption the behaviour of the carbon rods, between which an electric arc passes. The electric light has its seat, as is known, in the brightly glowing surface of the positive carbon. From this the carbon is carried to the negative pole. If two carbon rods which are not too thick are placed about 1^{mm} from each other, and a very strong current is allowed to pass between them, a quick passage of the carbon is observed from the positive to the negative pole, and the latter increases as quickly as the other diminishes. The consequence is that the intervening space moves forward without becoming notably greater. This is explained by the fact that the carbon during its transport through the arc cannot burn, since the narrow space between does not admit of the penetration of air or only in very small quantity. The electric arcs working with direct current require to be so regulated that the arc is exactly of the right length to burn all the carbon carried over. In this case it can be clearly seen by the use of a dark glass, that it is the frequently changing position of the positive carbon surfaces, from which the electric arc proceeds, which are so brilliantly luminous. It is therefore not, as is often assumed, the striking of the pieces of carbon torn off and carried by the arc upon the negative pole which actually produces the light. This production of heat at the point of separation of the loosened from the solid carbon can only be explained by the carbon being carried through the electric arc in the metallic form, that consequently the latent heat of the carbon is set free at the point of separation, and this is consequently highly heated.

CONTRIBUTION TO THE THEORY OF ELECTRO-MAGNETISM.*

THE question that induced me to take up this enquiry was what influence the magnetism already existing in the iron of an electro-magnet, or produced simultaneously in another sense by external forces, exerted on the amount of the magnetization.

Ampère's theory demands the adoption of such an influence, if, with Wilhelm Weber, one supposes that magnetism, in conformity with Müller's experiments, always fully exists in magnetic bodies, although in limited quantity. But if there is only a limited number of elementary magnets, or of equivalent solenoids, in the iron, a magnetizing or directive force cannot have the same effect, if a directive force acting vertically to it, simultaneously acts rotatively on the elementary magnets. For maximum magnetization this directly follows from the consideration that two forces, acting simultaneously on a mass of iron, which tend to magnetize it in two directions at right angles to one another, can always be replaced by a third force acting in the direction, and with the strength, of the resultant of these forces. The magnetization of the mass of iron must therefore take place in the direction of the resultant of the magnetizing forces, and must attain its maximum in this direction. The magnetic moment of the elementary magnets, pointing in the direction of this resultant, must therefore amount to $\sqrt{\frac{1}{2}}$, in the direction of the effective forces here assumed to be equal. This must at least be the case when the magnetized iron body is a sphere, and the maximum of magnetization in the direction of the resultant of the forces is actually reached. For masses of iron of various dimensions, this consideration becomes complicated by the difference of the mutual reinforcement of the magnetism which the magnetized iron molecules exert on one another, to which I will recur later.

This deduction from the Ampère-Weber theory is not yet confirmed by experiment so far as I know. This is partly due to the circumstance that the process of magnetization of magnetic bodies

* Monthly Report of the Berlin Academy of Sciences, 23 June, 1881.

is not yet made altogether clear in all directions, so that the experimental determination of a precise question is rendered very difficult, partly however in this particular question because it is difficult to eliminate the disturbing influence of the powerful magnetizing forces themselves on the measurement of a determined magnetic moment of the iron. To do this, it was necessary to make use of electro-magnets of special form, with which both the magnetizing force and the magnetism engendered by it in the iron in the one direction, was without influence on the indications of the measuring apparatus with which the magnetization in another direction was measured.

This condition is fulfilled by the use of a straight iron tube, which is so wound with insulated wires parallel to the axis, that the inner and outer wall surfaces of the tube are uniformly covered with parallel wires. Such a longitudinal winding—as used in the Pacinotti ring frequently employed in electric work—when traversed by an electric current, causes a magnetization of the whole wall of the tube at all points in the direction of the tangent to the tube, so that the tube represents a closed ring magnet. As Kirchhoff* has shown, such a closed ring electro-magnet exerts no external action. As regards the axis of the iron tube this follows from the consideration that all parts of the wall of the tube, as well as the longitudinal windings, lie symmetrically with respect to the axis, and that the magnetic action at a distance of opposite windings and magnetized iron parts are neutralized as regards it. If the longitudinally wound iron tube be surrounded by a second outer transversely-wound spiral which, traversed by a current, magnetizes the iron tube in the direction of its axis, the sum of the magnetic moments of the spiral and of the iron tube in this direction is to be measured by a mirror magnetometer placed in the axis of the tube, whilst a current through the longitudinal windings, and the tangential magnetism of the wall of the tube called into existence by it, remain without influence on the magnetometer.

In these experiments an iron tube 15^{mm} in internal diameter, 150^{mm} in length, and 3^{mm} thick, was used, which was provided with 36 longitudinal windings of copper wire 1^{mm} thick. The

* Pogg. Ann. Supplementary, Vol. V., p. 1.

longitudinally wound tube was placed in a wire coil 100^{mm} long, of 326 convolutions of similar wire. The tube projected about 25^{mm} from both ends of the coil. The action of the coil on the galvanometer was compensated by a second coil at a distance from the first, which formed a continuation of the wire of the first, so that both coils were always traversed by the same current.

If now the iron tube so surrounded was directed towards a magnetometer with a dead beat bell magnet, being at right angles to the meridian, and a current of about 10 Bunsen cells passed through the outer coil B, the magnetometer gave a deflection on the scale which constituted a measure of the magnetism produced in the direction of the axis of the tube. Then a battery of from 1 to 8 cells was simultaneously inserted, one after the other, in the inner (longitudinal) spiral A. The deflection of the magnetometer diminished in consequence, and this diminution certainly increased with the increase of the battery in circuit with the longitudinal coil.

The experiments were so arranged, that the deflection of the magnetometer was first read with the battery in the outer (transverse) coil without any current in the longitudinal coil, then stronger batteries were inserted one after another in the longitudinal coil, and the resulting deflections were observed.

As may be seen from this, the deflection of the magnetometer, corresponding to the current in B, diminished during the experiment, which evidently proceeds from the simultaneous diminution of the current in B. In the curve to Table I. (Fig. 38) these experiments are represented reduced to equally strong currents in B. (Abscissæ, strength of current in A ; ordinates, deflection of the magnetometer.)

It is thus proved, that the magnetism produced in a mass of iron by a magnetizing force is smaller, if at the same time it is magnetized by other forces in a direction at right angles to it. The reversal of the direction of the current in the longitudinal coil is moreover altogether without effect on the amount of the deflection.

The magnetism of a ring approaches its maximum even with comparatively weak currents. This proceeds in the first place from the total magnetizing action of a wire entirely surrounded with iron traversed by an electric current being very much greater

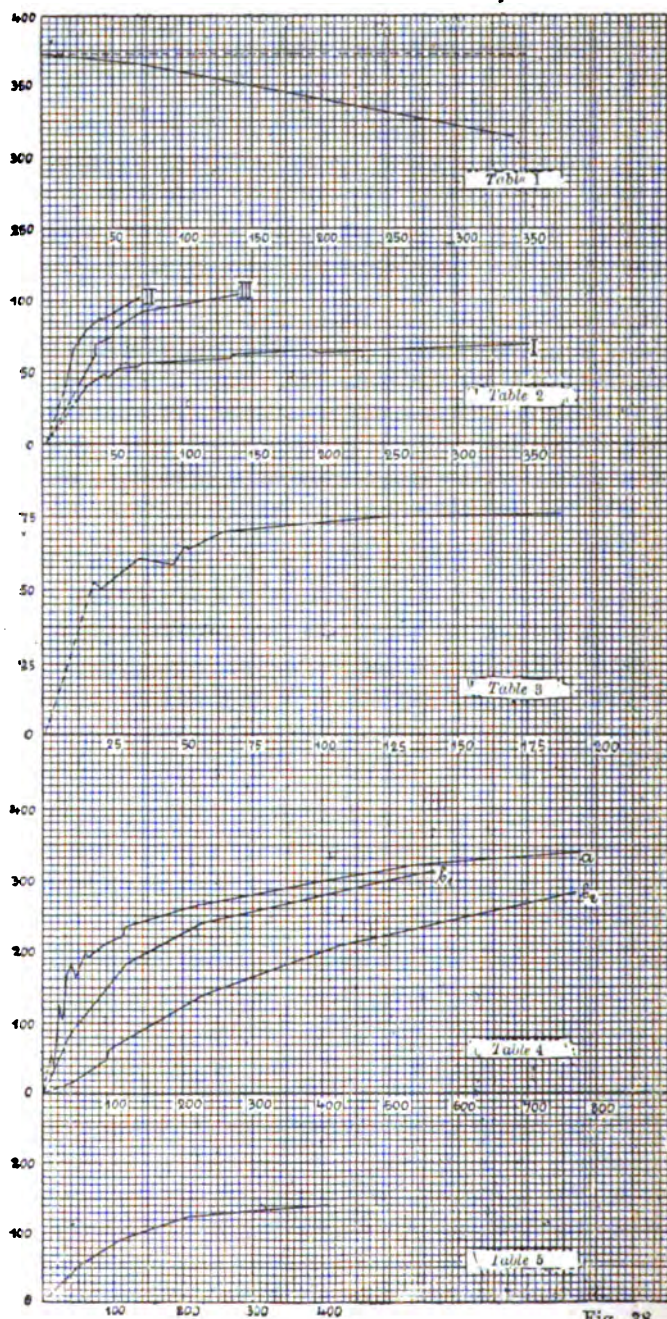
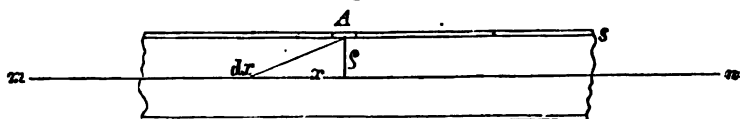


Fig. 38.

than when the same wire is wound on an iron rod, and further from the considerably increased action which closing the armature exerts on the magnetism in a short magnet. The magnetizing action of a wire assumed for the sake of simplicity to be of unlimited length, coinciding with the axis of the cylinder, may be determined by a simple calculation.

Let A be an iron tube of length l , radius ρ , and small thickness s . Further let $m n$ be the axis of the tube, which coincides with that of a straight conductor $m n$ of unlimited length. The element of current dx will then exert on a particle of iron situated in the wall of the tube of the dimensions ρda , s and dl a magnetizing force in the direction of the tangent to the tube, which if the

Fig. 39.



current strength is represented by i , and the circular angle by α , is expressed by—

$$i \cdot dx \frac{\rho}{\sqrt{\rho^2 + x^2}} \cdot \frac{\rho + d\alpha}{x^2 + \rho^2} \cdot s \cdot dl$$

or for the whole ring by—

$$2\pi \cdot s \cdot i \cdot \frac{\rho^2}{(x^2 + \rho^2)^{\frac{3}{2}}} \cdot dx \cdot dl;$$

The magnetism of the whole ring in the direction of its periphery is then—

$$2\pi s \cdot i \cdot dl \int_{-\infty}^{\infty} \frac{\rho^2 \cdot dx}{(x^2 + \rho^2)^{\frac{3}{2}}} = 4\pi s i dl;$$

and as the same action takes place for all rings along the whole length of the tube, the magnetism of the whole tube for the length l has the value—

$$M = 4\pi l \cdot s \cdot i.$$

As the value of ρ no longer appears in this expression, the

diameter of the tube has no influence upon the magnitude of the magnetism produced. The total magnetism produced in the iron wall of a tube by a central unlimited conductor is therefore independent of the diameter of the tube, and directly proportional to its length and to the thickness of the wall.

In order to prove the accuracy of this calculation, three iron tubes of equal length but different thickness and diameter were prepared, and each of the tubes was provided with two longitudinal coils. The primary coil of each tube consisted of 90, and the secondary of 30 windings. Through the primary coil alternating currents were sent, and the induced currents produced in the secondary coil by the reversals of magnetism were measured by the deflection of a mirror galvanometer. The dimensions of the iron tubes *a*, *b*, *c*, of 100^{mm} length were—

	Inside diameter. mm.	Wall thickness. mm.
(<i>a</i>)	10·8	2·3
(<i>b</i>)	11·0	4·5
(<i>c</i>)	17·4	4·5

The results of the experiment are contained in Table 2 and the curves belonging to it in Fig. 38; in the curves the abscissæ represent the strengths of the current, the ordinates the magnetism. As may be seen from diagram I, in which the horizontal abscissæ are the measured current strengths, the vertical the deflections produced by the corresponding induction coils, the magnetism so measured is about proportional to the thickness of the walls, whilst the greater inside diameter exerts indeed a diminishing influence, which is however inconsiderable, and is explained by the method of measurement. Exact agreement cannot of course be expected by means of these experiments, because the nature of the iron has an important influence in the case of electro-magnets.

So far only the direct magnetizing action, which a current coinciding with the axis of the ring exerts on the iron tube, is taken into consideration, not the strengthening action which the elementary magnets or solenoids diverted from their position of equilibrium by the current in the direction of magnetization exert on one another, and thus increase the magnetism. It is difficult to account for this strengthening molecular action, which

plays so important a part in electromagnetic phenomena, if the Ampère-Weber view is adhered to that the molecular magnets are situated in all possible directions with their centres at a uniform distance. It is also hardly conceivable, and to my knowledge it has never been sought to be proved, that on this supposition the action of the limiting layers of the body of any shape is altogether without influence, and that in no portion of an unmagnetized iron body an action at a distance of the molecular magnets can occur. This difficulty is removed, and at the same time a very clear explanation of many electromagnetic phenomena obtained, if the Ampère-Weber theory is modified by assuming that each molecule of iron consists of two elementary magnets with opposed poles lying opposite one another, which together are freely rotatable in every direction without expenditure of work, whilst the molecular magnets of each pair are turned away from each other by external magnetizing forces, as would be the case with an astatic pair of needles, if the needles could move independently in their parallel planes of oscillation. If the distance apart of the elementary magnets is taken as small compared with the distance of the paired molecules, an action at a distance of the iron masses not magnetized by outer forces cannot occur even at the surfaces of the body. But if on the other hand there is a directing outer force, it must turn the two elementary magnets of the paired iron molecules in different directions, so that all the north poles are turned in one, all the south poles in the opposite direction. If there were no action of the iron molecules thus magnetized on one another, the couple, which as a magnetizing force tends to turn the elementary magnets of one molecule away from each other, would have to be equal to the force with which the elementary magnet poles, moved from their position of rest, act on one another in opposition to the rotation. But there occurs besides a simultaneous attraction between the opposed poles of all the elementary poles so directed, and a repulsion between all similar poles, the resultant of which produces a strengthening of the rotation directly produced by the magnetizing force. This increasing interaction only takes place in the direction of magnetization as the interactions of the molecular magnet groups lying near one another balance each other. The phenomenon of residual magnetism or magnetic coercive force,

as well as the heating of the electromagnet by frequent rapid reversals of polarity, require the further assumption that there is a frictional resistance opposed to the rotation of the elementary magnets with respect to one another, whilst the paired molecules as assumed can turn without resistance in every direction. This frictional resistance limits the mutual strengthening of the rotation of the elementary magnets, and prevents on the other hand the complete disappearance of magnetism after the discontinuance of the external magnetizing force.

By the adoption of this modification of the Ampère-Weber theory many magnetic phenomena hitherto unintelligible find a simple explanation. According to it the magnetism of a bar of iron on all the molecules of which an equal magnetizing force is exercised, must increase with the length of the bar, until a state of equilibrium has been reached between all turning and frictional moments of the whole of the molecular magnets lying consecutively in the direction of the magnetization.

The middle of the bar must hence be most powerfully magnetized, and here an approximation to the maximum of the magnetization must occur soonest. Further, a thinner bar must become more strongly magnetized by equal forces acting on it, and must therefore earlier approach the maximum of magnetization than a thicker one, for with thin bars all the molecular magnets reinforcing one another lie more directly behind one another, the total action must therefore be greater. As the molecules of the end surfaces of the electromagnet bar are only exposed on one side to the action of the molecular magnets increasing the magnetism, the end surfaces of short bars must possess half the magnetism of the middle of the bar, plus that due to the direct action of the magnetizing force. That this latter direct rotation is small in comparison with that of the simultaneously existing reinforcement, follows from the strong magnetization of short closed ring or horse-shoe magnets by weak magnetizing forces. With such a closed ring magnet the magnetizing of each section of the ring must be proportional to that of the section through the middle of a very long magnet bar, because in the ring the strengthening as well as the outer magnetizing action is the same in each section. The magnitude of the magnetization of a closed ring magnet is therefore dependent

firstly on the maximum of the permeability of the iron, and secondly on the sum of the friction resistances of the molecular magnets of the whole circuit. The strengthening action must therefore diminish with the length of the iron bent into a ring with equal magnetizing influence on all molecular magnets through external forces. From the experiments described above, therefore, the wider tube *C* should with equal magnetizing forces have taken less magnetism than the narrower tube *b* with equal thickness of wall. As already follows from the above mentioned experiments, and as is made more certain from the later ones, a proportionately weaker current in the magnetizing coil already suffices to bring the ring magnetism near to the maximum magnetization. Consequently the simultaneous strengthening of the magnetism of the molecular magnets must considerably surpass the direct magnetization by the external magnetizing force. This is also confirmed by the fact that a thin disc of iron which is laid on the poles of a powerful magnet, is not attracted by it to any extent if the edges of the disc do not extend beyond the polar surfaces, but that a powerful attraction instantly takes place if a portion of the iron plate extends beyond the edge of the polar surfaces.

That the sustaining power of closed horse-shoe magnets increases according to some observers as the square of the magnetism, and according to others at least in a much higher ratio than the magnetism itself, appears to be opposed to this view. But as follows from the following experiments, the sustaining force of a short ring or tube magnet is nearly directly proportional to the effective magnetism measured by induction. That this must be the case follows from the consideration that the magnetic attraction of two sections of the ring, infinitely near to one another, must be equal to the sum of the simultaneous attractions of all magnetized molecular magnets on both sides of the section, but that this sum of all the attractive forces is also to be considered as the active magnetism in the ring section. The anomalous observations are to be explained by too great a length of the magnetic circuit, by incomplete contact of the armature and magnet surfaces, and by too small a size of the touching surfaces.

A tube magnet of 10.8^{mm} inside diameter, 2.3^{mm} thickness and 150^{mm} length was divided into two half cylinders by a section

passing through the axis of the tube. The halves of the tube were carefully ground to one another, and each surrounded by one-half of the two coils of wire. By suitable arrangements the weight could now be determined, which was necessary to tear the halves of the tube from one another, and simultaneously the induction current produced in the induction coil by the separation was measured. In the following Table II.

TABLE II.

a) IRON TUBE 1.

Thickness = 2.3^{mm}; Diameter, 10.8^{mm}.

Primary Current.	Secondary Current.	Tearing away Force in Kilogr.	Secondary Current. Weight.
23.0	30.2	10.3	2.92
32.4	39.4	12.6	3.11
44.4	44.0	14.2	3.10
51.6	49.5	16.5	3.00
69.0	53.7	17.0	3.16
133.8	61.1	20.8	2.94
195.0	63.6	23.5	2.71
248.0	66.3	27.3	2.43
296.0	68.2	28.5	2.39
343.0	69.5	31.5	2.21
297.0	68.7	28.5	2.41
241.0	66.6	26.5	2.51
190.0	65.7	26.2	2.51
131.0	58.5	24.8	2.36
68.2	55.0	17.0	3.24
52.2	51.2	17.2	2.98
41.6	47.0	15.0	3.13
30.8	40.5	12.5	3.24
19.6	27.9	9.7	2.89

b) IRON TUBE 2.

Thickness = 4.5^{mm}; Diameter, 11^{mm}.

Primary Current.	Secondary Current.	Tearing away Force in Kilogr.	Secondary Current. Weight.
17.0	44.7	11.6	3.85
31.0	78.1	20.1	3.89
41.0	86.5	25.0	3.46
63.0	101.1	61.0	1.66
69.0	100.8	59.8	1.68
40.0	87.3	28.3	3.08
22.0	65.0	17.5	3.71

c) IRON TUBE 3.

Thickness = 4.5^{mm}; Diameter, 17.5^{mm}.

Primary Current.	Secondary Current.	Tearing away Force in Kilogr.	Secondary Current.
			Weight.
24.0	38.9	10.5	3.70
36.6	63.3	24.3	2.60
47.0	75.0	34.2	2.19
68.0	89.1	41.2	2.16
140.0	104.0	53.3	1.95
140.0	103.6	51.5	2.01
71.0	91.2	38.3	2.38
50.4	81.3	32.0	2.54
37.2	69.5	27.3	2.54

(pp. 362, 363), the first vertical column contains the strength of the current in the magnetizing coil, the second the induced current set up by the separation, the third the separating weight in kilogrammes, the fourth the quotients of the numbers in the last two columns. These quotients in the fourth column should all be equal if the sustaining force were directly proportional to the active magnetism. As may be seen, considerable deviations exist, and the quotients somewhat diminish with increasing current strength. But this can also be ascribed to the greater compression of the surface layers, to bending and to other mechanical causes.

A more suitable form is given to the tube magnet by bending the iron tube into a circle. If the annular hollow space, circularly enclosed with iron, is filled with a suitably wound coil of wire after the annular tube is divided by a cut through the greatest plane of the ring into two equal semi-circular rings, and it is thus made possible to lay the coil of wire inside, the formula deduced above for the magnetizing and sustaining power can be used without great error for this annular tube magnet if the radius of the ring is not too small.

Table III. gives the tearing away experiments made with such an annular tube magnet.

The two ring-shaped iron channels which when laid on one another formed the tube magnet were well ground to one another. To each channel a brass stirrup was attached, by means of which they could be torn away from one another. The spiral consisted of 360 convolutions of covered copper wire .5^{mm} thick, having 8.7 units

TABLE III.

Number of Elements.	Primary Current.	Secondary Current.	Tearing away Force in Kilogr.	Secondary Current.
				Weight.
1	20.5	50.4	31.5	1.6
2	46	58.4	42.3	1.4
3	52	63.8	46.8	1.6
4	68	69.6	47.5	1.5
5	82	71.5	49.2	1.5
6	93	72.7	51.4	1.4
8	116	73.9	57.8	1.3
10	139	76.0	58.5	1.3
20	183	77.4	65.2	1.2
4	65	69.0	49.3	1.4
3	51	66.0	45.3	1.5
2	34	61.3	37.8	1.6
1	18	52.6	29.4	1.8
1 shunted	12	46.0	24.9	1.7
1 „	8.3	38.3	18.0	2

resistance. Its inner diameter was 62^{mm}, the outer 81^{mm}, its section was consequently a circle of 86^{mm} diameter. The thickness of the iron channel was 2^{mm}. To measure the magnetism produced in the tube magnet, 50 convolutions of fine insulated wire were wound up together with the coil of wire, so that this consisted of the principal coil described, and of a secondary coil, which were insulated from one another. The principal and secondary coils were firmly fixed to the upper iron channel, so that the lower iron channel formed the armature to be separated. The motion, after the tearing off was limited to a few millimetres by a projection on a rod passing through the ring and fixed to the stirrup of the lower annular channel.

A strongly damped mirror galvanometer was now connected with both coils through a suitably arranged commutator in such a way, that with the commutator in one position with the help of a shunt to the principal coil, the current strength of the latter could be measured, in the other position the current induced in the induction coil by the tearing apart. The tearing apart took place by the lower portion of the rod fixed to the stirrup of the armature being also provided with a projection which allowed of disc-shaped leaden weights with slots reaching to the middle of the disc being slipped on to the rod, these being then held by the projection. If the sustaining power of the magnet was nearly balanced by the

application of the requisite number of such weights, then a spring balance also fixed to the rod of the armature was slowly stretched, and the weight indicated at the moment of rupture registered, while another observer noted the deflection of the mirror galvanometer, which indicated the current produced in the induction coil by the separation. This deflection is a measure of the magnetism which has disappeared in the magnet by tearing off the armature, and therefore a measure of the increase of magnetism by closing the armature. In order to obtain the whole effective magnetism existing in the magnet before rupture, the deflection must be taken account of which occurs on interrupting the magnetizing current after the deflection produced by the induction of the principal coil itself on the induction coil is deducted from it. The weights necessary for separation are nearly proportional to these numbers. The deviations can be sufficiently explained because magnetism still remains in the iron of the magnet on open circuit, and because in spite of the careful grinding all the particles of iron on both sides of the cut surface still make imperfect contact. The stronger the pressure of the surfaces on one another, the more perfect must be the contact.

As follows from Table III., the maximum observed sustaining power is 65.2 kilos. The maximum of the sustaining power reckoned from the increase of this power would be about 75 kilos. The weight of the iron tube bent into a ring amounted to 192.54 grammes, the weight of the wire coil to 180 grammes. One gramme weight of iron magnet and armature taken together, therefore bore 328 grammes, and on the above assumption for the maximum of magnetism the sustaining power was 390 times the whole weight of the iron.

In the apparatus described the alteration was then made of providing it with 12 external bobbins, which were placed in halves on the closed ring, and then coiled with insulated wire. The inner width of the bobbin was about 5^{mm} greater than the thickness of the ring, so that the tearing away of the half-rings from one another could be effected without being prevented by the bobbins. The bobbins were then wound with similarly insulated wire to that used for the interior principal coil. Two opposite bobbins were connected up as induction coils, the remainder constituted a principal coil for producing magnetization of the ring, the direction of which must

be everywhere perpendicular to the direction of the magnetism of the tube produced by the principal coil. The amount of the ring magnetism produced could be measured by the deflection which was caused in the induction coils by closing the outer principal coil. This deflection only gives, it is true, the amount of magnetism existing in that portion of the ring enclosed by the induction coil, therefore through portions of the ring in which none or only a little direct magnetization is produced by the principal coil, but it may without considerable error be taken as a measure of the whole magnetism produced in the ring, both because as already noticed the direct rotation of the elementary magnets by the outer magnetizing force is only small as compared with the mutual strengthening of the molecular magnets, and also because the diminution of the progression of the magnetization through small lengths of soft iron of sufficient section is not considerable.

A battery was now inserted in the outer transverse principal coil. By altering the connections suitably there was measured on the same mirror galvanometer, first the deflection caused by the induction coil, and then the strength of current in the principal coil; this was repeated many times, the direction of the current in the principal coil being reversed each time. The deflection produced by the induction coil then constituted the measure of the magnetism produced in the ring by the current strength I .

If the current of the principal coil was now allowed to continue in one of the two magnetic circuits, and the principal coil of the other circuit was then closed, a deflection was obtained in the induction coil of the latter, which indicated a reduction of the magnetism produced in this circuit. The result obtained with straight tube magnets was thus confirmed, viz., that the magnetization of the iron by an external magnetizing force is smaller when a simultaneous magnetization exists or is called forth in a direction perpendicular to it.

During the first part of the experiments the strength of the external current was about 800, during the second about 200; those experiments, in which this strength of current varied from the numbers named, were reduced to the number 800 or 200, upon the supposition that the action of the outer current is proportional to the strength, which appears admissible with the slight variations.

TABLE IV.

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
Lower Half Ring Attached.	Inner Current Closed.	Strength of Inner Current.	Outer Current Closed.	Strength of Outer Current.	Outer Current Interrupted.	Inner Current Interrupted.	Lower Half Ring Detached.	Sum of Induction Deflections.	Magnetism during Inner Current.	Magnetism during Inner and Outer Current.	Remanent Magnetism.
+ 7	+ 28	+ 11	- 30	+800	+ 28	- 18	-20	- 5	+ 35	+ 5	+15
+ 9	+ 93	+ 26	- 93	...	+ 87	- 57	-54	-15	+102	+ 9	+39
+11	+149	+ 46	-143	...	+132	- 92	-72	-15	+160	+17	+57
+13	+176	+ 63	-158	...	+147	-113	-79	-14	+189	+31	+65
+12	+185	+ 71	-160	...	+149	-120	-81	-15	+197	+37	+66
+12	+202	+ 92	-168	...	+152	-133	-83	-13	+214	+51	+70
+12	+199	+ 92	-145	...	+159	-131	-82	+12	+211	+ 66	+94
+12	+208	+113	-145	...	+157	-148	-78	+ 6	+220	+ 75	+84
+13	+251	+221	-131	...	+140	-181	-86	+ 5	+263	+132	+91
+12	+290	+424	- 92	...	+ 96	-220	-84	+ 2	+302	+210	+86
+12	+327	+766	- 54	...	+ 57	-256	-83	+ 3	+339	+235	+86
+10	+ 51	+ 10	- 37	+200	+ 37	- 31	-33	- 3	+ 61	+ 24	+30
+10	+115	+ 21	- 76	...	+ 81	- 73	-61	- 3	+126	+ 49	+58
+11	+153	+ 33	- 91	...	+ 95	- 96	-72	0	+164	+ 73	+72
+12	+170	+ 44	- 90	...	+ 94	-110	-74	+ 2	+182	+ 92	+76
+11	+185	+ 58	- 83	...	+ 88	-121	-76	+ 4	+196	+113	+80
+12	+221	+115	- 55	...	+ 57	-156	-77	+ 2	+233	+178	+79
+12	+254	+219	- 31	...	+ 31	-186	-77	+ 3	+266	+235	+80
+11	+287	+395	- 17	...	+ 17	-218	-77	+ 3	+298	+281	+80
+12	+313	+561	- 11	...	+ 11	-242	-71	+12	+325	+314	+83

Further, the increase of the magnetism produced by the outer primary coils alone was observed by measuring the deflections occurring in the outer secondary coils. In this case no induction resulted from the putting on and off of the under half of the iron ring. The induction deflections are those obtained after frequently closing and opening the current in one direction : those obtained on first closing, especially with weak currents, were somewhat greater, yet never more than about 5%. (See Table V.)

TABLE V.

Primary Current.	Secondary Current.
3	3
56	56
114	92
221	124
410	144

If there was a current in the inner coils, and the current in the outer coils was reversed (+ closed, opened - closed, opened, &c.),

and observations taken with the inner secondary coils, the first deflection was about 2 % greater than those that followed.

Hence the remanent magnetism corresponding to the external current alone appears to be considerably less than that corresponding to the internal current.

In Fig. 38, *a'a* represent the curves of magnetism corresponding to the internal current alone (Table IV., column 2), b_1 and b_2 the curves for the simultaneous action of the external current, b_1 for the current strength 200, b_2 for the current strength 800 respectively.

The following was the detailed arrangement of the experiments :

First of all the increase of magnetism was observed in a direction perpendicular to the centre line of the ring, by the action both of the inner current (windings in the direction of the centre line) and of the outer current (windings perpendicular to the centre line) ; as a measure of this magnetism the induced current in an inner secondary coil (windings in the direction of the centre line) was taken ; the deflections contained in the following table are, with the exception of columns 3 and 5, all those observed in the inner secondary coil. There were measured in the following order (see Table IV.) :

1. The deflection on attaching the lower half of the iron ring to the upper ;
2. the deflection on closing the inner current ;
3. the strength of the inner current ;
4. the deflection on closing the outer current ;
5. the strength of the outer current ;
6. the deflection on opening the outer current ;
7. the deflection on opening the inner current ;
8. the deflection on removing the lower half of the iron ring.

If it be assumed that no magnetism or a perfectly constant remanent magnetism exists after the opening of the currents and the removal of the lower half of the ring, the sum of all induction deflections must be nil ; this is indeed, with a certain approximation, the case, as column 9 shows ; column 10 (1 + 2) gives the magnetism produced by the inner current, column 11 (1 + 2 + 4) that by the inner and outer currents ; column 12 (1 + 2 + 7 + 8) the remanent magnetism corresponding to the inner current.

From the weakening influence referred to which the remanent or simultaneously produced transverse magnetism exerts on the

amount of magnetization, many phenomena are explained which act in a disturbing manner both on scientific electromagnetic experiments as well as on the technical application of magnetism.

The straight or annular tube electromagnets used for the above experiments are distinguished from the construction of electromagnet hitherto employed by producing a much greater magnetic effect than the latter for a given weight of iron and copper. They will therefore find a very frequent and useful application, especially in the electric arts. The property of the annular tube magnet of surrounding the conducting wire completely with an iron cover makes it specially applicable for answering scientific problems, for the solution of which the necessary means were hitherto wanting. Such a question, for instance, is the screening action of iron. It certainly appeared probable, and was also generally assumed hitherto, that magnetic induction was not directly influenced by an interposed iron screen, and that the observed alteration of the magnetic induction was to be explained by the action of the magnetism produced in the interposed iron screen. But decisive experiments on this point have not been made so far as I am aware, and this was hardly practicable with the methods hitherto known.

To determine the question by means of the annular tube magnet, I had two such magnets made as similar as possible, and arranged them on both sides of the bell magnet of a dead-beat mirror magnetometer in such a way, that I fixed the two coils by means of stretched wires to an upright frame, which could be brought as near to the magnet as desired. The same current was now passed through the two coils in series, and one of the frames was moved, until no deflection of the magnetometer took place on opening and closing the current. The one or other coil was alternately covered with the two halves of the tube so as to form a closed tube magnet, and the deflections of the magnetometer produced on closing the current measured in divisions of the scale. The experiments showed that in fact a decided, although only slight, lasting reduction of the magnetic moment of a coil occurred when it was quite surrounded by an iron tube. The amount of this screening action can be measured by approaching the weakened coil. It is apparently proportional to the thickness of the tube wall, but this requires further proof. I

will here only remark that a magnetic induction of the iron, if such could be assumed to be produced with an annular ring magnet magnetized as a tube magnet, must produce a strengthening and not a weakening of the induction of the coil. I hope to find later an opportunity of going more fully into this question, and abstain in the meantime from any explanation of this remarkable phenomenon.

This actual, although only slight screening, action of iron brought me to the question if by aid of the tube magnet it could not be determined whether magnetic induction acts directly and immediately in a straight line as has been assumed for gravity, since the time of Newton, or whether it acts progressively from molecule to molecule of the intermediate chain or of the hypothetical æther, as was first assumed by Faraday for electric induction, and proved by me to be admissible by experiments.*

In fact, a certain probability appeared to exist that the magnetic force passing forth from a coil of wire could not simultaneously perform a considerable work, the magnetization of the closed tube magnet in the closed tube wall which must surround it, and at the same time exert an unimpaired induction. It appeared more likely that the induction would commence beyond the tube wall when the work to be performed on traversing the iron in rotating the elementary magnets was accomplished. The experiments have not confirmed this conjecture. The same arrangement of two divided annular tube magnets was used with slight alteration, with a mirror magnetometer placed between them, as was employed in the above described experiments. First of all the parallel and equal coils of wire set up on both sides of the magnetometer were so arranged that a current, which passed through both in series, showed no effect on the magnetometer. Then one of the two coils, without altering its position, was covered with its iron screen, and the experiment was repeated. There was now no definite deflection of the magnetometer, as should have been the case if the current in one coil acted longer or more strongly on the magnetometer than in the other. As the time difference was as short as possible, and consequently its effect was negligibly small in comparison with the strong total action of each coil, I

* Pogg. Ann., Vol. CII., p. 66.

modified the experiment at the suggestion of Dr. Frölich (whom I have to thank for conducting this experiment, and those formerly described), in such a manner that in place of the magnetometer a third uncovered coil was set up, and the external, also uncovered coils were so arranged, that no current was induced in the middle coil by them. For its measurement the charge of a mica condenser was used, with the two coatings of which the ends of the wire of the middle coil were connected. My frequently described hammer for the production of currents of very short duration was now so inserted that a strong current circulated continuously through both coils. One of the two removable pins of the hammer now interrupted this current, whilst the second, after a very short time, interrupted the circuit of the middle coil and the condenser. As the middle coil consisted of a large number of windings of fine wire, a very slight difference in the magnetic moment of the two outer coils must have caused a measurable charge of the condenser. As by the interruption of one connecting wire between the middle coil and the condenser, this latter was insulated, and in this condition held a charge for some minutes without appreciable loss, as was proved by experiment, the later discharge of the condenser through a sensitive mirror galvanometer must be a measure of the difference of potential existing at the ends of the convolutions of the middle coil, at the moment of the interruption of the condenser wire. By this arrangement of the experiment it was certainly not exactly the retardation of the beginning of the induction of the coils of wire enclosed in the iron that was measured, but, as it were, its complement, namely, the supposed strengthening of the magnetic induction of this coil by a cessation of the magnetization of the iron of the tube magnet after breaking the current. But it may well be assumed that this action must occur, if the supposed retardation of the induction were occasioned by the magnetization, as otherwise energy is lost. These experiments also gave negative results. At least the differences observed were so small and uncertain that they could not be considered as decisive.

The experiments last described have incidentally confirmed in a striking and simple manner Helmholtz's theory of the discharge of a condenser by a series of alternating discharges and renewed charges. If an uncovered coil is alone allowed to act on the

induction coil, and between each two experiments the duration of the connection of the condenser with the induced coil is increased, the discharge deflections of the condenser, which in the first instance were positive, become negative. By further increasing the duration of the connection they were again rendered positive, and so forth. At the same time the deflections steadily decrease.

ON THE LUMINOSITY OF FLAME.*

THE light that radiates from burning gases, which shine with a bright flame, is, as is known, a secondary phenomenon. It is from the solid or even fluid particles suspended in the flame which are separated by the high temperature of combustion, and brought to incandescence, that the bright rays of light radiate. Gases which separate no solid or fluid particles on incandescence, or produce none during the process of combustion, generally burn with a relatively slightly luminous flame, of a bluish or other colour according to the sort of gas being consumed. It is customary to explain the cause of this luminosity by the fact that the gases themselves become luminous when highly heated by combustion. No experiments have hitherto been published so far as I know as to whether highly-heated pure gases actually give out light. Considerations regarding the radiation of light from the sun, which enter into the solar theory of my brother, C. William Siemens, and upon which I intend shortly to make a communication to the Academy, together with some occasional observations, have made it appear to me improbable that heated gases are self-luminous, and I determined to make some experiments on this point. In order that the experiments may be of a decisive character they must be made at temperatures higher than those which are produced with luminous combustion. I soon satisfied myself that on this, as well as on other grounds, satisfactory results could hardly be expected from experiments made in the laboratory. On the other hand, the regenerative furnaces in the

* Monthly Report of the Berlin Academy of Sciences of 9 Nov., 1832.

glass works of my brother Frederick Siemens, at Dresden, appeared to me specially suitable for the performance of such experiments. My brother willingly acceded to my wish that an experiment should be made with such a furnace, and he found my anticipations fully confirmed. A regenerative furnace used for the manufacture of hard glass according to my brother's method was employed, situated in a separate space which could be made quite dark at night. The furnace had a rectangular hearth about $2\frac{1}{2}$ metres in length and $1\frac{1}{2}$ in breadth, and its greatest internal height was about 160^{cm}. In the centre of each of its long sides there were openings opposite to one another, which permitted a clear view through the furnace. The furnace could easily be heated to the highest temperature which could be borne by the furnace walls, built of specially refractory fire brick; this is the temperature of fusion of steel, which lies between 1500° and 2000° C. After this temperature was reached, and the further admission of gas and air into the furnace was stopped, its hot walls maintained the temperature of the interior of the furnace fairly constant for a long time, if all admission of air was prevented. In front of the openings of the furnace were placed a number of well blackened screens with a central opening, which allowed a view through the heated furnace, without the rays radiating from the walls affecting the eyes. After the furnace was thoroughly covered all over, and all light was removed from the space, so that complete darkness existed in it, there was not the least appearance of light visible to the eye from the highly heated gases in the furnace. If a luminous flame was brought into the space, the reflection from it sufficed to slightly illuminate the field of view. For the success of the experiment it was necessary to prevent all combustion in the furnace, and to wait until the gas in the furnace was as free as possible from dust. Any flame in the furnace, even when it did not reach within the line of sight, or the least quantity of dust in it, illuminated the field of vision.

As the result of these experiments of my brother, it must be assumed that the previous view that highly-heated gases are self luminous is not correct. In the furnace there were the products of previous combustion, mixed with atmospheric air, therefore oxygen, nitrogen, carbonic acid, and steam. If only one of these gases were self luminous, the field of view must always have been

illuminated. If gases are, however, not self luminous at the temperature of combustion, then the feeble light given by the flame of burning gases which separate no solid or fluid particles cannot be explained as the glow of the heated products of combustion. It then also appeared to me probable that heated gases would radiate heat rays as little as light rays. In order to make an experiment on this point, and to convince myself by my own observation of the correctness of the phenomenon observed by my brother, I went to Dresden with Dr. Frölich. As to the illumination of the heated furnace gas we obtained in general the same result as had been obtained by my brother and his engineer, Mr. Hermann, who had carried on the experiments with great zeal and intelligence. Certainly the field of view was not always quite dark, and there was often only a short time for observing it. With the great sensitiveness of the eye sharpened by the prevailing darkness, and the impossibility of preventing every motion of the air combined with motion of dust, as well as of preventing all admission of gas, this is easily explained. We have, however, repeatedly proved complete darkness of the field of view. Unfortunately experiments could not be carried out by means of sensitive thermopiles to decide the question of the emission of rays of heat. The small size of the openings in the furnace, as well as the considerable distance from the furnace, at which the thermopile would have had to be placed, diminished the exactness of the measurement, so that no difference could have been observed between the luminous flame and the heated air.

I however found out later, by means of a different and perfectly simple experiment, that my assumption was erroneous. An ordinary gas lamp, with a ring-burner, and short glass chimney was so screened by a thick board placed in front of it, that the board concealed the whole of the lamp as well as the glass cylinder. A sensitive thermopile was so arranged that the axis of the tube in which it was placed, lay a little higher than the upper edge of the board. The tube was provided with a screen, and could be turned about a vertical axis. As the walls of the room had about an equal temperature, the deflection of the sensitive mirror galvanometer inserted was only inconsiderable when the axis of the tube was so placed, that the hot current of air rising from the flame did not lie in the field of vision of the thermopile

limited by the screen. But if the latter were so turned, that the line of sight fell on the hot stream of air, then a deflection immediately took place, which only returned when by turning the thermopile further, or by turning it back, the heated current of air was removed from the field of vision. The same result was obtained when the lamp itself was shifted behind the board concealing it, and alternately brought into and removed from the field of view. That the radiation of heat from hot gases is only very small compared with that from equally hot solid bodies, is shown by the great deflection of the galvanometer which occurs when a piece of fine wire or any other solid substance is brought into the current of hot air. On the other hand, however, it is much too considerable to be able to assume that the heat radiation was only brought about by dust particles suspended in the current of air.

The question naturally arises whether the radiation of light from heated gases, in the same way as the radiation of heat is not extremely slight, therefore easily to be overlooked when the temperature is not very high. This possibility must anyhow be conceded, and it is very desirable that experiments should be repeated at yet higher temperatures, and with better appliances, to fix the limits of temperature at which heated gases are undoubtedly self-luminous. The fact that gases at a temperature of more than 1500° C. are not yet luminous, shows, however, that the luminosity of flame cannot be explained as a self glowing of the products of combustion. A consideration of the flame itself supports this view. When care is taken to quickly mix the gases which are to be burnt, the flame becomes shorter, because the process of combustion goes on more quickly, and at the same time is of a higher temperature, because less cold air is mixed with the burning gases. In the same way the flame becomes shorter and hotter, when the gases are well heated before burning. As the ascending products of combustion still retain for some time the temperature of the flame, the opposite effect would happen were the gases self-luminous. But the luminosity of flame ceases at a sharp limiting line above it, and coincides with the completion of the chemical action. This, therefore, must be the cause of luminosity and not the heating of the products of combustion by it. If it be assumed that the gas molecules are surrounded with an envelope of æther,

then on chemical combination of two or more such molecules an altered position of their æther envelope must occur. The motion of the æther molecules thus brought about must be balanced by vibrations which may form the starting points of the waves of light and heat. In a quite similar way the light phenomena may be explained which always happen when an electric current is passed through gases. As I explained* long ago in describing the Ozone apparatus, all gases became conductors of electricity when what I have called the polarization maximum belonging to them is exceeded. This proves that the dielectric is only able to carry a quantity of electricity depending on its nature, *i.e.*, with gases on their density, and that with a further increase of the difference of potential the phenomenon occurs of the passage of electricity through the dielectric. If the production of sparks is prevented in an air condenser, as in the ozone apparatus, by a plate of glass or mica placed between the condenser plates, then a glow takes place in the whole body of gas in the layer of air on passing beyond a fixed potential difference dependent on the distance of the plates and on the density of the gas, which is repeated on the discharge of the condenser. The gas has thus become a conductor of electricity for this difference of potential, and the dielectric of the condenser now consists only of the glass or mica plate, which has a much higher polarization maximum than the gas, and, therefore, becomes a conductor much later. As the current conducted by the gas always appears to be connected with chemical action, the luminosity could also be explained in the same way as with flame, by an oscillatory shifting of the æther envelopes of the gas molecules by means of which the passage of the electricity is effected. The light of flame might then be called electric light with the same propriety as the light of the ozone-tubes or Geissler's tubes, which only differs from the first principally by containing a dielectric of very slight maximum polarization. The similarity of the appearance of the flame in strength and colour is also in favour of the agreement of the cause of the luminosity of flame and of gases through which the electric current is passed.

* On electrostatic induction and the retardation of the current in cores. *Pogg. Ann.*, Vol. CII., p. 66.

ON THE ADMISSIBILITY OF THE ASSUMPTION OF
AN ELECTRICAL SOLAR POTENTIAL, AND ITS
IMPORTANCE FOR THE EXPLANATION OF
TERRESTRIAL PHENOMENA.*

MY brother, Sir William Siemens, in his paper "On the conservation of solar energy," set up the hypothesis that the sun possessed a high electrical potential, which probably produced the luminous phenomenon of the zodiacal light. He accounts for the source and maintenance of this electrical potential by friction of the matter, which, according to his theory, after dissociation by the rays of light and heat from the sun, streams into its polar regions from space. This would be reburnt after condensation had taken place, and then flow towards the sun's equator. In this way it would be electrified by friction with the rotating body of the sun, and would then be again diffused throughout space in the electrified condition, by the centrifugal force of the sun's rotation.

If this much-disputed theory of my brother be admitted as correct, the phenomenon is really similar to that described by me† of the electrification of the apex of the Cheops pyramid by the whirling aloft of the dust of the desert. It could then be assumed that one of the electricities separated by friction was retained by the body of the sun considered as conductive, and as insulated from the photosphere, the sea of flame, surrounding it, whilst the other was diffused through space by means of convection. As it must then also be assumed that this convection extends far beyond the orbits of the planets, the body of the sun must have an electrical potential as regards them, and act inductively upon them.

I will not enter further into the controversy as to the admissibility of my brother's theory. I do not fail to see the importance of many of the arguments brought against it, but I am of opinion that the assumption of an electrical potential, which this theory makes possible is highly in its favour, since some of the most important terrestrial phenomena could thus find an explanation

* Excerpt from the Berlin Academy of Science, 31 May, 1883.

† Pogg. Ann., Vol. CIX., p. 355.

hitherto sought in vain, and on the other hand in the present state of scientific knowledge hardly any other explanation for the existence of an electrical solar potential can be found. For so far no process is known to us by which one electricity only is called forth; we know only separations of the two electricities, and although such separations would most probably occur on a very large scale, owing to the very powerful mechanical and chemical actions on the surface of the sun, yet they must be again balanced by conduction within it, and even if a separation of both electricities was kept up in the body of the sun, no inductive action of one of them could take place. So long as no new and hitherto unknown facts are brought forward, my brother's convection theory is inseparable from the assumption of an electrical solar potential.

I must not, however, omit to consider the very important objection brought against this theory by Messrs. Faye and Hirn. It is that the unchangeability of the periods of revolution of the planets around the sun logically excludes the assumption of a space filled with matter. Astronomical observations unconditionally require the assumption that space is absolutely vacuous, as owing to the great velocity of the planets, even the most rarefied atmosphere would cause a measurable diminution of the velocity, and consequently a shortening of the periods of revolution of the planets. This would be correct on the supposition that the atmosphere of space is relatively at rest. This cannot, however, be the case, if the circulation assumed by my brother actually takes place. The sun's atmosphere must be assumed to have nearly the same period of revolution as the sun's body. Any difference in the velocity of rotation, brought about by the powerful ascending and descending currents in the sun's atmosphere which must arise from the combustion of the sun's elements cooled by expansion, and the cooling by radiation of the burnt outer layers of the photosphere, will be again equalized by continual friction against one another of the portions of the sun's atmosphere rotating with different velocities. As yet nothing is known as to the height of this atmosphere rotating at the same rate as the sun. According to Ritter's calculations,* indeed, the density of the sun's atmosphere, following the sudden alteration of the adiabatic curve, diminishes very quickly in the

* Wied. Ann., Vol. V., pp. 405, 543.

region of the atmosphere, in which the supply of heat produced by combustion considerably retards the reduction of temperature, corresponding to the progressive rarefaction. We do not yet know, however, the limit of rarefaction up to which the law of Mariotte and Gay Lussac holds good. If the atmosphere however reach the limit, where the force of attraction balances centrifugal force, each material particle beyond this boundary must henceforth rotate as a planet around the sun. If new particles of matter constantly pour into this space, the material must become more and more compressed, and a ring be formed which would rotate around the sun according to Kepler's law. But if a continuity of the sun's atmosphere is assumed this ring formation cannot take place, as the mutual friction of the layers of gas extends even beyond the surface of equilibrium, and there is consequently an acceleration of the layers of gas already in planetary motion. Hence with the increase of velocity the distance of all these microplanets from the sun continually increases, and consequently that constant outflow from the sun's atmosphere into space must actually take place, which my brother assumes. This can only take place in the zone of the sun's equator, for here the centrifugal force at equal distance from the sun's centre is greatest. The density of this atmosphere everywhere rotating in accordance with Kepler's law in the plane of the sun's equator must be assumed to be constant even at great distances from the sun, since solar gravitation is everywhere balanced by the velocity of rotation. In directions perpendicular to the plane of the sun's equator on the contrary the density must decrease, for the sun's attraction diminishes as the distance from the plane of the sun's equator increases.

It follows from this consideration that a material current emanating from the sun coincident with the phenomenon of the zodiacal light must everywhere have the same period of revolution as the planets which are at the same distance from the sun. A resistance experienced by the planets from the material parts of interplanetary space which move nearly uniformly with them round the sun, is therefore out of the question. Only a resistance (here negligible) must take place in consequence of the inclination of their ecliptic to the plane of the sun's equator, which perhaps accounts for the observed diminution of this angle of inclination. The satellites also in revolving round their planets must experience a resistance

from the atmosphere of cosmical space, whilst the outermost layers of the atmosphere of the planets, rotating with them, must suffer a frictional resistance. As regards the moon, Mr. Hirn is perhaps right when he asserts that at the great velocity of the heavenly bodies, even the most rarefied resisting medium would carry away their atmospheres.

Numerous observations render it very probable that space at least within the region of the solar system is filled with combustible material. This also is indirectly very decidedly in favour of my brother's assumption, that the products of combustion in a state of great rarefaction and at a very low temperature are again dissociated by the solar rays. The objection which has been made that the work of dissociation would absorb the energy of the luminous rays, and thus render cosmical space opaque, may be set aside by supposing that it is only the invisible chemical rays which accomplish the work of dissociation. But it can also be assumed that the work of dissociation has already been accomplished in the course of ages, and that now only the chemically combined mass continually emitted from the sun has still to be dissociated by its rays of light, which would only require a portion of its luminous energy. It would not be easy to explain without the hypothesis of dissociation why cosmical space is not filled like the atmosphere of the earth with oxygen, nitrogen, and hydrogen. It cannot be assumed that the composition of the body of the sun is essentially different from that of the earth if both have been produced from the same cosmical rotating nebulous mass, for we cannot assume a separation of matter in the gaseous state according to specific gravity. Hence, in the solar system at least, the electronegative material must everywhere predominate, and it is to be assumed that in the future the cold burnt-up sun will be surrounded by an atmosphere containing oxygen in excess. But if cosmical space is filled with dissociated products of combustion in a state of great rarefaction these must be affected by the sun's attraction, except where, as in the neighbourhood of the solar equatorial plane, they are withdrawn from its attraction by planetary rotation. There must therefore be a continuous inflow of dissociated matter towards the sun as my brother assumes, especially in the polar regions where centrifugal force is altogether absent. If the sun's mass, as may well be assumed, remains

unchanged, it means that a state of equilibrium has been attained in which as much burnt material streams out from the sun in its equatorial zone as is drawn again into its polar regions in the dissociated state by attraction. Hence also would follow the current from the pole to the equator as well as the proved lower angular velocity of rotation of the gaseous mass of the sun in its higher latitudes.

But although this makes the production of an electrical solar potential possible by friction and the continual removal of portions of matter from the sun charged with one electricity, the mechanism of this electrification still remains very obscure. The light of the sun proceeds from a sea of flame which must have according to Ritter's beautiful calculations a height of about 25 kilometres. Whether a flame of burning gases of this thickness will still transmit many heat and light rays of a source of emission of higher temperature, and how much of them it will absorb or reflect like a cloud layer, we cannot know. I have lately proved * that gases heated to from 1500° to 2000° C. still appear quite dark, although radiating the more slowly vibrating heat rays at a lower temperature. Whether gases become self luminous at yet higher temperatures has not yet been determined by experiment. But as a small flame in a brighter light casts a shadow, it appears unlikely that many of the light and heat rays from the deeper and hotter solar strata can traverse the thick photosphere. The observed temperature and light of the sun are then phenomena, originating essentially in chemical activity, which goes on in the sun's atmosphere. This requires that the sun's atmosphere rising in the dissociated state and simultaneously cooled by increase of volume shall begin to burn when the limit of the temperature of dissociation for the respective compound is exceeded, and that this combustion shall continue until the loss of heat by expansion is equal to the sensible heat of combustion. The apparent temperature of the sun will therefore be approximately the dissociation temperature, and particularly of those compounds which have the greatest chemical heat equivalent, such as that of water, the elements of which will burn at the greatest height, whilst the heavier masses possessing a higher temperature of dissociation already burn in lower regions.

* Berl. Ber., 1882, p. 961; Wied. Ann., Vol. XVI., p. 311.

To maintain this upflow of the dissociated elements of the sun and the combustion connected therewith, the final products of combustion must return to the body of the sun. As Faye, Ritter, Reye, and others have already explained, this takes place, first, because the products of combustion have a greater specific gravity than the unburnt gases ; and, secondly, in consequence of the cooling of the higher strata of the photosphere by the radiation of heat and light. In this way the adiabatic equilibrium of the different layers becomes disturbed, and the higher having become relatively heavier, return in descending currents to the solar depths. The reason why these descending currents become visible as sun spots only in the middle latitudes of the sun is, that there only the conditions for a rotatory motion of the descending current exist, by which a vertical direction is given to it. The funnel-shaped diminution of the diameter of sun spots is due to the great diminution of volume with rapidly increasing pressure. The interior of the funnel must be relatively dark, for no luminous flame is formed there, as the temperature must be lower, by the amount of the heat of dissociation, than that of the surrounding unburnt solar substance, and perhaps products of condensation have already made their appearance, which keep back like a screen the radiation of the deeper more highly luminous layers of the sun. On the other hand, it is not unlikely that the solar flames bursting forth consist of bubbles of hydrogen and oxygen in the proportion to form explosive gas, or of oxygen mixed in proper proportion with coal gas, which in consequence of their less specific gravity and greater liberation of heat in combustion bursting through the penumbra and photosphere rise up high, and the elements of luminous flame being absent partly transmit the rays of the hotter deeper layers of the sun. The enormous velocity of some of the solar flames, hardly to be accounted for mechanically, could then be explained by this radiation from the sun's depths. My brother in a recently published supplement to his theory of the sun, assumes that the body of the sun could not itself be hotter than 3000° C., since at a higher temperature the chemical rays would preponderate, and at much higher temperatures the sun would even cease to be luminous. This would be correct if the photosphere did not act as a screen to keep back the hotter rays of the sun's body as it probably does. Indeed, from analogies of observa-

tions, we cannot draw any safe conclusion as to whether a body heated to hundreds of thousands or millions of degrees will still be luminous. Only rays of so small a wave length might emanate from it that they would be incapable of performing any chemical work. The apparently dark nucleus of sun spots might then be explained by the flameless products of combustion, relatively cooled by incipient dissociation, returning to the sun, remaining transparent, and permitting radiation through them of deeper layers of the sun too highly heated for luminous radiation. The violet colour of the nuclei of sun spots would tell in favour of this. For attainable temperatures certainly the law holds good, that besides the rapid undulations of the æther corresponding to the higher temperature, the whole range of the slower also make their appearance ; but it cannot be known whether it is not otherwise with so very much higher temperatures.

It was necessary to consider more closely the probable constitution of the sun's body and the envelope from which its light and heat radiate in order to obtain some basis on which to found an answer to the question, whether in the present state of our knowledge, the hypothesis of an electrical solar potential appears admissible. As I have already contended, its possibility is only conceivable if a separation of both electricities takes place on the sun's surface, and if at the same time one of the separated electricities is conducted away. As flame is a good conductor of electricity, the whole photosphere, and the penumbra (which also probably takes part in the process of combustion), may be regarded as a conducting covering surrounding the hotter body of the sun. As besides flames have the property, like points, of transferring electricity to what surrounds them, in this case therefore to their gaseous products of combustion, the photosphere must be continually discharged by a partial outflow of the products of combustion into cosmical space. If therefore the photosphere were insulated from the deeper body of the sun not yet involved in the combustion, the latter, if considered as a conductor of electricity, could be charged with electricity by frictional or chemical processes acting between the conducting body of the sun and the photosphere. The question whether hot gases are conductors of electricity, even when not luminous, has not yet been proved by direct experiments. That gases, like all bodies, become conductors

of electricity when the dielectric polarization of their molecules has reached a maximum, and that this maximum diminishes proportionately with the rarefaction of the gases, consequently also with their temperature as measured from the absolute zero, I have already shown in the year 1857, when describing my ozone apparatus. Conductors therefore only differ from insulators in the maximum polarization of the former being vanishingly small. That in very highly heated gases, as in metallic conductors, the polarization maximum would be vanishingly small can hardly be admitted. I know of no direct experiments on the dielectric properties of highly heated gases rich in flame, but the phenomena of the electric spark as well as the luminous appearance in the ozone apparatus and in Geissler's tubes, as well as the beautiful experiments of Hittorf,* can be explained even without supposing that the conductivity of very highly heated gases differs from that of cold gases of equal density. Hence the high temperature of the solar gases appears at present to be no obstacle to ascribing insulating properties to them. Indeed their polarization maximum in correspondence with the density of the sun's atmosphere, will be greater than that of our cold atmospheric air, notwithstanding their high temperature.

Quite different relations, however, may occur with the appearance of the critical state at greater depths in the sun. We have neither experiments nor analogies to go upon as regards the electrical property of the critical state, and may therefore assume

* Mr. Hittorf, in a communication in Wiedemann's *Annalen*, Vol. XIX., p. 73, says that my communication made to the Academy on the 9th Nov., 1882, that gases at temperatures of from 1500° C. to 2000° C. appear perfectly dark if quite free from flame, and the luminosity of gases on passing an electric current through them is a similar process to the shining of a flame, which separates no solid elements, had previously been made known by himself and others. I willingly allow this as regards the non-luminosity of heated gases, and besides I made no claim of priority in this communication, yet I believe that I first showed experimentally that such highly heated gases actually appear quite dark even when the heated layer of air is more than a metre thick, and the eye has become sensitive in the highest degree by complete darkness. The Hittorf experiments only proved a relative darkness of heated gases. As regards the conductivity of gases (which Faraday assumed for high tensions), I may say that in my paper above cited I had twenty-five years previously already given the general law which the conductive capacity of gases follows. To this paper I might also refer Mr. E. Wiedemann, who claims the priority of having shown that the luminosity of gases by the passage through them of an electric current is a result of dielectric polarization.

the interior of the sun to be a metallically conducting mass, *i. e.*, with an exceedingly small polarization maximum. The surface of the sun's mass in the critical state might then have an electrical potential. The question however would have to be considered whether the conducting photosphere might not become electrical by induction, on the face turned towards the interior of the sun, so that the sun with the photosphere surrounding it would form a very powerful Leyden jar, by which induction of the electricity of the conducting solar nucleus would be in a great measure excluded. This cannot be at once assumed, as the conductivity of flame evidently depends on causes directly connected with the combustion process itself, and therefore quite distinct from that of conducting bodies not in chemical action, so that hardly any analogy can be inferred between them as regards their capacity for electrical induction. I have therefore made some experiments as to whether a flame is affected in the same way as other conductors by inductive action, and these experiments have confirmed the hypothesis. Hence two flames insulated from each other may act in the same way as other conductors as the coatings of a charged Leyden jar.* It must hence be assumed that the seat of

* The experiment was so arranged that a ring-shaped gas burner was insulated. On opening a gas cock a cylindrical flame of about 2 centimetres diameter rose to a height of about 15 centimetres. The flame passed through an insulated metallic cylinder of about 8 centimetres diameter surrounding it concentrically. Conductive connection with the flame was effected by an insulated platinum wire of circular form placed in the lower part of the flame. The charge between this platinum wire and the metal cylinder due to a galvanic battery of 50 Daniell cells was then measured with my quickly oscillating electro-magnetic switch with the gas cock alternately nearly closed and quite open. The difference between the deflections of the mirror galvanometer was then a measure of the capacity of the Leyden jar formed by the flame and metal cylinder. The results obtained are collected in the following table :—

Number of Oscillations of Commutator per Minute.	Difference of Deflection of Scale between Lower and Higher Flame.	Amount of a Discharge.
310	3	96
600	6	100
700	8	115
1000	12	120

The increasing figures of the last column show that with slow oscillations a portion of the charge was lost by conduction.

the sun's electricity is to be sought chiefly in the photosphere and not in the body of the sun. The electrical properties of flame are still very obscure notwithstanding all the experiments hitherto made on the subject, and in particular it has not yet been decisively determined, whether a difference of potential spontaneously produced exists or not between the different zones of flame, especially between that where the combustion begins and where it ends. If this were the case, as appears probable from experiments by Riess and others, the cause of the sun's electricity might be sought therein, with the enormous dimensions of the sea of flame surrounding the sun and the correspondingly great differences in temperature and density, since the electricity of the outer layers of the photosphere would pass over to the products of combustion and with them be partly transferred to space in the direction of the plane of rotation of the sun, according to my brother's hypothesis. But even if the process of electrification has to be sought in solar combustion, in the friction of matter flowing in from cosmical space, or in other causes yet unknown, the possibility of the existence of an electrical solar potential is given by the equatorial diffusion of solar products of combustion in cosmical space.

But this possibility rises to the rank of great probability when one considers how easily difficult and still unsolved problems of terrestrial phenomena can be solved with the help of a solar electrical potential. If the sun possesses a high electrical potential it must act inductively on all heavenly bodies, and therefore also on the earth. But an accumulation of one kind of electricity on its whole surface can only occur when the opposite electricity liberated is conducted away, and this is only conceivable by diffusion in cosmical space. The process is approximately the same as takes place when a charged spherical conductor is placed opposite to a little insulated ball. The ball then gradually takes an opposite charge whilst the similar electricity is lost by dissipation into space. As regards the earth the dissipation of the so-called free electricity due to the sun's induction is greatly favoured by the extreme rarefaction of the upper strata of air and by the ascending and descending currents of air charged with moisture, as by these the free electricity is conveyed to the upper strata of highly rarefied air, the occurrence of electric currents in

which is proved by the zodiacal lights. They might be considered as the electrical compensation taking place on the limit of the earth's atmosphere between the materials flowing out from the sun, charged with negative electricity and the liberated positive induced electricity of the earth. This balance must always take place when by a change of the sun's potential that of the earth is also changed. For the restoration of the balance positive or negative electricity must flow out from the earth; therefore a balance must take place at the boundary of the atmosphere with the negative electricity pouring out from the sun, or this must flow to the earth. The reason why this interchange takes place specially at the polar surfaces of the earth may be that the polar air is more strongly electrical, as it is continually displaced by the more strongly electrified air brought by the equatorial current of air in the upper regions of the atmosphere, and consequently receives the whole mass of the electricity of the highest strata of air of the lower latitudes. Earth currents being intimately connected with the northern and southern lights are then to be considered as a necessary consequence of the compensation of the variations of intensity of the sun's and earth's electricity taking place, especially in the polar regions. These compensation currents must on their side affect the magnetic needle by their electrodynamic action.

But the question here obtrudes itself whether the earth's magnetism itself is not to be considered as an electrodynamic action of the electric charge of the earth. According to the interesting experiments which Mr. Rowland has made in Helmholtz's laboratory under his direction, it may be considered as proved that static electricity when mechanically moved produces electrodynamic effects in the same way as an electric current. Hence, if the surface of the earth is charged with electricity of great density, magnetic phenomena must be exhibited in consequence of its rotation, just as if electrical currents circulated around it, carrying round it in each latitude during the period of revolution as much electricity as the static electricity found on the respective surface rings. It will not be difficult for skilled mathematicians to calculate what must be the density of the electricity on the earth's surface in order to produce by its rotation the earth's magnetism. As the magnetic moment of a circular current is in proportion to the surface surrounded it may be seen that it

will not be inadmissibly great, taking into consideration the great dimensions of the earth. Further, on account of the colossal dimensions of the sun, the surface of which is 11,483 times that of the earth, whilst its distance is only 22,934 times the earth's radius, the density of the sun's electricity needs only to be about twice as great as that of the earth to call forth the latter by electrical induction. If the whole surface of the earth were equally charged with electricity the magnetic poles would agree with the earth's poles of rotation. As this is not so, and as generally great irregularities take place in the distribution of the earth's magnetism over its surface, the distribution of the static electricity on its surface must also be irregular. This also appears probable when one remembers that about one-third of the earth's surface consists of land, and that greatly of rock, often only thinly covered with badly conducting earth. The accumulation of the induced electricity will consequently have to be sought rather on the surface of the incandescent well-conducting interior of the earth, by the greater distance of which from the earth's surface the preponderating influence of the nearest lying masses of electricity found in convective motion is diminished. Whether it will be possible to deduce the existing distribution of the earth's magnetism and its observed secular and irregular disturbances from this theory of the cause of the phenomenon of terrestrial magnetism will depend on later special investigation. The daily regular disturbances might be explained by the density of the induced electricity on the side turned away from the sun being somewhat less than on the side turned to it. This unequal density of the earth's electricity, dependent on the position of the sun, must proceed with the rotation of the earth, and may therefore be the cause of the regular equatorial earth currents discussed by Lamont. The magnetic disturbances of the moon may likewise find their explanation in the reaction of the lunar electricity upon the distribution of the induced electricity of the earth. On the other hand the secular alteration of the position of the magnetic poles can only be attributed to unknown cosmical causes.

But if this theory still leaves much to be explained, it at least affords the possibility of giving an explanation of the origin of the earth's magnetism in agreement with our previous experience. This is not the case with any previous theory. The hypothesis

of a central magnet in the interior of the earth is contradicted by the universal experience that a red heat destroys the magnetism of all bodies, and cannot be maintained without quite giving up the foundation of experience. The hypothesis of a stratum of magnetic ore in the earth's crust as the seat of the earth's magnetism is contradicted in the first place by calculation, for the magnetism of such a stratum, even if assumed to be of the greatest possible thickness and magnetized to a maximum, would not suffice to produce the existing magnetism of the earth; and again there is the impossibility of finding a cause for the magnetization of this stratum of ore, since it cannot have been in existence from the beginning but must have first arisen after the cooling of the earth. The same could also be maintained of the theory set up after Faraday's discovery of the magnetic properties of oxygen, that the oxygen of the air was the seat of the earth's magnetism, if calculation had not already shown that the seat of this magnetism cannot be outside of the earth's surface. Just as little can the theories of the earth's magnetism meet with consideration which depend on thermo-electric currents, or as Zöllner attempted to base his, on convection currents in the interior of the fluid earth, since in a medium conducting equally well in all directions such currents cannot occur. Besides no cause can be found for the existence of permanent regular currents of the fluid interior of the earth.

Just as an electrical solar potential affords the possibility of explaining the earth's magnetism and the phenomena connected with it of the northern and southern lights and earth currents, it also serves to explain atmospheric electricity and thunder-storms. Lamont has already assumed that the earth must be charged with negative electricity to explain the varying and irregular atmospheric electricity. His opinion that this electrical charge was to be explained by thermoelectric differences is however as little tenable as the opinion that frictional processes could produce an electric terrestrial potential, which can only arise from cosmic influence and the removal of the similar electricity which becomes liberated by diffusion in space, or its neutralization by the oppositely charged matter which flows out from the sun in the direction of the plane of the sun's equator. But if this is assumed to be the case, that consequently the earth forms with the sun a

condenser whose separating dielectric is the atmosphere of the sun and of the earth and the interplanetary space filled with highly rarefied matter, all the conclusions drawn by Lamont and others from the electric charge of the earth are justified. But to account for the electricity of thunderstorms, the trifling and variable electricity of the atmosphere to which it has hitherto been attributed does not seem sufficient. It cannot be assumed that the sudden production of the enormous quantity of electricity which occurs, especially in tropical storms, originated in the feeble electric charge of the comparatively small quantity of air that carried the thunder-clouds. The sources from which it arises must be more productive. Such a source of inexhaustible vastness is found in the electrical charging of the earth by solar influence. When a conducting body is brought near to a large electrically charged sphere it is subjected to the inductive action of the electricity on the surface of the sphere. If the positive electricity collected in that part of the conductor furthest from the sphere finds a means of transmission to still more distant conductors, the first conductor becomes permanently charged with electricity, the polarity of which is opposite to that of the sphere. But if the elevation of the conductor above the surface is only slight in proportion to the diameter of the sphere, then the difference of tension between the surface of the sphere and the furthest point of the elevation can only be small. On this account, even with a great density of the electricity on the earth's surface, no electrical repulsion can take place there, and even on mountain tops it cannot be very considerable. But the relation is otherwise when a sphere is charged by induction from a distant electrical sphere. The lines of force passing, according to Faraday's molecular distribution theory, from the charging to the charged sphere from which the electricity is led away, meet the latter everywhere almost perpendicularly, and with a great distance between the influencing spheres acting upon each other in proportion to the diameters of the spheres, in nearly equal number on the side turned to as on that turned away from the distributing sphere. If now an insulated conductive screen covering a portion of the surface of the influenced sphere is brought near to the latter, it will not, if thin, become perceptibly electrical. But as soon as the screen is touched conductively it takes the electricity

opposite to that of the sphere, whilst that of similar sign is conducted away. The behaviour is inverted when the screen is itself conductively connected with the sphere, the screen then forms a portion of the surface of the sphere, and takes its electrical charge, while the portion below the screen becomes non-electric. Thunder clouds on the earth's surface form screens of this character. If such a cloud screen is supposed to form a portion of the earth's upper surface it will remain unaffected by the earth's electricity, so long as the conducting particles of water are insulated and at a great distance from one another. Hence mist and light clouds do not become electric. But as soon as the mist has so far condensed that its conducting portions come into contact with one another, or the distance between them becomes so small that electricity of very slight tension can spring over the intervening space, the cloud is subjected to an induction process. This can be brought about by its being put into conductive connection with clouds in very high regions by ascending cloud vortices. This cloud conductively connected is then electrified in its lower part with electricity opposite to and in its upper part with electricity similar to that of the earth. But a dense conducting bank of cloud may also come in one or more places into conductive connection with the earth itself. Then it forms a portion of the earth's conducting surface, and takes up its electricity.*

* During a voyage on the Mediterranean in the neighbourhood of the Spanish coast between Carthage and Almeria in the winter of 1865, I had an opportunity of observing the course of the phenomenon of a water spout, which appears to me to tell decidedly in favour of this conception.

Between the ship and the coast in the vicinity of Almeria, with a so-called dead sea which was considerably agitated without much motion of the air, appeared a dense but apparently not high bank of cloud under which the sea appeared to be in the wildest commotion. It had the appearance of a white round spot, the diameter of which the seamen estimated to be 2 or 3 nautical miles, foaming high up, whilst the surrounding sea had only smooth waves without any breakers. Notwithstanding the considerable distance of the ship from the place of agitation, probably several nautical miles, it could be clearly seen through the telescope that the wild surge of the sea rose many metres above the clearly defined surface of the relatively smooth sea. The clouds descended at one place in the shape of a funnel, forming a streak of cloud curved like an elephant's trunk, which reached down very nearly to the foaming surface of the sea, and branched out somewhat below. Perfect contact with the foaming surface could not be perceived, and what seemed surprising the sea did not foam any more under the cloud trunk than in other places. The trunk itself slowly rotated, if I remember rightly, in the direction of the hands of a watch over the white spot, and its junction with the cloud also took part in this motion although not to the same extent.

The latter process would most easily occur on the slopes of steep mountains, against which the layers of cloud rest ; hence mountains frequently cause storms. Electricity does not appear to play any essential part in the production of clouds which carry thunder storms. The cause of the formation of clouds is to be sought as a rule in the ascending and descending motion of the air, to which not only this, and the rain falling from the clouds, but also the letting loose of storms is almost exclusively to be ascribed. The various views on this subject still prevailing in meteorology appear to me to require correction on some points. If the equilibrium of the sea of air was not constantly disturbed by unequal heating and cooling of the air by radiation, the temperature and density of the air up to the greatest heights could not but be in so-called indifferent equilibrium, and in such a way that the loss of temperature with increasing height would be everywhere equivalent to the work of expansion of the gas. The higher temperature of the air of the lower latitudes would be balanced by slowly travelling whirlwinds with a horizontal axis of rotation, as is shown on a large scale by the trade winds, and finally the whole sea of air would possess equal temperatures at equal heights. This indifferent or adiabatic equilibrium is now continually disturbed by extra heating of the earth's surface, and of the lower strata of the air by solar radiation, by absorption of the same on passing through the atmosphere, and by the extra cooling of the higher strata by radiation outwards. The lower

Unfortunately, after about half-an-hour's observation, during which the trunk had made about a revolution and a half, its point remaining constantly at about one-third of the radius of the white spot from its margin, the approach of night and the increasing distance prevented a further contemplation of this interesting phenomenon, which was followed with the greatest attention by myself, my brother William and his wife, and the naval officers belonging to the French cable ship on board of which we were. No whirling motion was perceptible, and there was almost a dead calm. The phenomenon could only have been a purely electrical one, and must have consisted in an electric current from the earth to the cloud. If this current is assumed to have become so strong at one place that the water was electrically carried up and a conducting water communication formed between sea and cloud, the rotation of the trunk under the influence of the earth's magnetism is also explicable. During the night a storm raged on the Spanish coast, the origin of which we had probably observed in the water spout ; the latter appears afterwards to have travelled from the Spanish to the African coast, for towards the end of the night, our ship, which was near to the African coast, was overtaken by so fearful a storm that it was in the greatest danger. The storm lasted but a few minutes, and the seamen were firmly of opinion that the water spout had passed over the ship.

strata would thus become lighter and the upper heavier than the adiabatic equilibrium requires, and this disturbance must be equalized by ascending and descending currents in the atmosphere. As the ascending air which has become warmer on the ground, corresponding to the adiabatic curve of temperature, preserves this surplus heat on ascending, but the upward impulse increases with the increasing height of the ascending current of air, because the succeeding layers of air on the ground have always the same excess of temperature, the upflow must continue wherever it has once been produced by specially favourable local conditions, until the difference of temperature is equalized. The work performed by the upflow of the relatively lighter air, and the downflow in other places of the relatively heavier air cooled by radiation, must be converted into *vis viva* as it sets the air in quicker motion. This is effected essentially by the increase of volume of the ascending air with diminution of its pressure. As the air space becomes only slightly greater with the height, owing to the great diameter of the earth, the velocity of the ascending air must on this account increase in nearly the same proportion as the diminution of pressure. In the highest regions of the atmosphere up to which each upcurrent will reach, the velocity of the air must therefore be very considerable, and with the same velocity the surrounding calm air must there be pushed aside to make room for that which has arrived. This displacement will chiefly take place towards the direction where a descending current of air has been formed to replace the superheated air flowing along the ground to the place of the upcurrent. The descending air becomes denser with descent in proportion to the height to which it had risen, *but at the same time it retains the velocity it had in the upper regions.* It is evident that a very great velocity of the air on the surface of the earth may finally result if the disturbance of the adiabatic equilibrium was qualitatively and quantitatively considerable. These local storms, the direction of which is modified by the rotation of the earth, according to Dove's law of rotation must be specially violent if the upcurrent itself is confined within narrow limits, for then the compensating process, *i.e.*, the conversion of the energy accumulated in the disturbance of equilibrium into velocity of air, is confined to a proportionately small quantity of air. But strong storms extending over whole continents can also

be produced by ascending currents of air of great local extent. That the descending current of air produces an increase of pressure on the surface of the earth, and the ascending a diminution follows from the laws of mechanical motion. But the motion of the air itself must always cause a fall of the barometer, as the air in motion carries with it the motionless air at their boundary of contact, and consequently produces a rarefaction. The final result of the balance of the disturbance will therefore be to put greater and greater masses of air into whirling motion, and finally to transform *vis viva* into heat by means of friction.

It follows from these considerations that the aqueous vapour in the air does not play that great part in its motion usually attributed to it, as the phenomena of the motion and pressure of the air can be explained without the water contained in the air. Only the origin of the storms, *i.e.*, in this case the place of acceleration of the masses of air must be sought not at the earth's surface, but essentially in the highest regions of the air. If the atmosphere consisted of aqueous vapour only, the phenomena would be just the same. Steam obeys the adiabatic law of expansion just as air does, only the density and temperature diminish with increasing height much less with steam than with the permanent gases of the atmosphere. According to Ritter an atmosphere of steam would be thirteen times as high as one of air. According to Clausius and Sir William Thomson, a continuous condensation, it is true, always occurs with the adiabatic expansion of steam, but at the heights in which, according to experience, the formation of clouds occurs, it must be too small to bring about the observed precipitation. The cause of the condensation taking place in ascending air currents lies essentially in this, that the steam is intimately mixed with the air and that in the ascending current it does not take the adiabatic temperature belonging to it, but that of the greatly preponderating mass of air with which it is mixed. Now as the air is cooled much more quickly than the steam with increase of height, this is cooled below the adiabatic temperature belonging to it; and this diminution of temperature causes its condensation if the point of saturation of the steam be exceeded.

To this conception is apparently opposed the circumstance that aeronauts have frequently proved that strata of warmer often

overlie those of colder air, whilst the law of adiabatic expansion requires a constant diminution of the pressure and temperature. But this is easily explained by the varying nature of the earth's surface, owing to which the ascending current of air varies as regards temperature and hygrometric condition at different times and places. If the hygrometric condition of such a hot ascending mass of air is so great that the water is partly separated during ascent, and falls as rain, the air met with in the upper strata of the atmosphere is still further heated by absorbing the latent heat of the aqueous vapour; its volume and buoyancy are thereby increased; the final result must be a stratum of relatively warm and comparatively dry air, which is then moved forwards by flowing expansively over colder but moister, and therefore lighter air.* These deviations from the rule that the temperature and density of the atmosphere diminish with increasing height, while the hygrometric state increases are easily explained. The latter must be the rule at least for higher latitudes, since the masses of warm air continually rising in the calms in a relatively humid condition on their way to higher latitudes, generally sink again to the earth as descending currents after loss of the greater portion of their heat by radiation, but must partly also reach the higher latitudes as upper equatorial currents. In this greater humidity of the higher strata of the atmosphere is to be sought the cause of rainfalls even with descending currents of air. If the temperature of a very moist upper current be cooled by radiation below the point of saturation of the vapour of water, cirrous clouds will be formed probably consisting of crystals of ice.† The latent heat

* Krönig has already proved that aqueous vapour mixed with an ascending current produces by its condensation no diminution but rather an increase of volume, as the latent heat of the steam increases the volume of the air much more than the volume of the condensed vapour. *Fortschritte der Physik*, Vol. XX., p. 626.

† It is, however, very likely that in the high regions of the atmosphere both water and steam retain their state of aggregation much below their points of freezing and condensation respectively. It is known that water can be cooled far below 20° C. without freezing if there are no solid bodies present in it to induce crystallization, and violent agitation is avoided. That steam in the same way can continue as steam below its point of condensation has not yet been proved experimentally. We know only of the retardation of the boiling point which gives occasion so frequently to steam boiler explosions. It is, however, not unlikely that this retardation of boiling corresponds to a retardation of condensation. It would be difficult to prove this experimentally for there are no means of cooling a mass of

of the steam and water set free in this manner will again heat these strata of air and protract the process of the formation of heavier snow-clouds ; but if the process is completed by continual loss of heat through radiation, the weight of the ice no longer filling any considerable space must disturb the adiabatic equilibrium and a sinking of the cloud mass commence. The condensation and heating taking place melt the snow again, and the necessary latent heat is withdrawn from the air. The adiabatic equilibrium is thus still further successively disturbed and the result will be a cold descending air current with rain. The density of these slowly descending rain clouds will not however be great enough to make the clouds electrically conductive and consequently no formation of electricity by induction can occur. But it is otherwise when by local overheating of the air near the earth's surface a local upcurrent with rainfall is produced. The upcurrent may then acquire a velocity greater than that of the drops of water falling in the resisting air ; these therefore will be whirled into the upper regions, the temperature of which is much below freezing point and be frozen to hailstones. With the rapid increase of volume and the corresponding lateral expansion of the accelerated current the next higher strata of air which are relatively wet and cold are set in motion about a horizontal axis of rotation, and combine with the whirlwinds ascending and rotating about a vertical axis. The violent whirling motion into which the hitherto calm overcooled sea of air is thrown will now bring on a sudden production of water and ice. The whirls with horizontal axis of rotation may thereby acquire a great diameter, and cast up the grains of ice again into the ice region until they become too heavy and fall to the ground as hailstones, or, after passing through lower warmer strata of air, as cold drops of rain. By this copious production of rain in a short time, the water particles of the path of cloud up to the higher strata of air are now brought so close together that it becomes a conductor of electricity, and is consequently exposed to electrical induction.

steam out of contact with solid or liquid bodies. Without this assumption it cannot well be explained why the sky is not always entirely covered with cirrus clouds, for it would have to be assumed, that water particles which have become fluid in the great rarefaction of the higher strata of air do not appear as clouds.

If at any part it is in conductive connection with the earth, the earth's electricity must flow into it, and it then receives similar electricity ; if it is not so, then it is charged in the neighbourhood of the earth with opposite electricity, whilst the similar electricity escapes by the conducting whirling cloud into the higher regions. Where the conduction of the cloud is imperfect, it is temporarily restored by lightning springing between the cloud layers insulated from one another, or between cloud and earth ; and finally on the whirling storm passing away and the cloud formed by it breaking up, the whole electricity will be brought into equilibrium through flashes of lightning with the earth's electricity, or will in part pass into the air as atmospheric electricity.

Many observations on the formation of thunderstorms have been made from the tops of high mountains or balloons ; in almost all of which numerous cloud layers are spoken of over each other which were in connection with one another or between which lightning flashes occurred. The most instructive description is that of Mr. Wite,* who observed the origin of a heavy thunderstorm from a balloon. "He saw two layers of cloud, one about 2,000 ft. above the other, from the upper of which snow, rain and hail passed to the lower. *Between the two passed noiseless, yellow, wave-like masses of light.* Electrical discharges with lightning and thunder occurred always in the lower layer, but the storm was much stronger *above both layers* than below them. A west wind set the upper layer in quick motion." That the observer could only see two clouds placed over each other is explicable, as his balloon was in the space between them. It may be assumed that there were several more such cloud layers up to the highest regions of the air, between which the observed precipitation and processes of electric conduction took place. The cloud layers were conductively connected with one another by the heavy rain pouring down from the upper cloud layer, especially from its centre, and thus submitted to the process of electrical charge and discharge.

It might be brought forward as an objection against the theory of an electric solar potential, that the electric attraction between the sun and planets, and the repulsion which the latter must exert

* Fortschritte der Physik, 1852, p. 762.

on one another and on their satellites would modify the basis of astronomical calculations, for then besides gravitation another force, viz., electricity would have to be considered.

This objection is quite correct. But as electric force like gravitation, varies in proportion to the square of the distance of the attracted points, the courses of the planets would remain unaltered, if a portion of the attraction of gravitation were replaced by that of electricity. Only the calculated ratio of the masses of the sun and planets to that of the earth would be altered. These alterations would be specially noticeable as regards the smaller planets and the satellites, since the electric force is a function of the surface. On the other hand, the disturbing influences which the planets and their satellites exert on their respective orbits must be changed if gravitation is diminished by electric repulsion.

Perhaps it will be reserved for astronomy to determine from the perturbations of the orbits of Mercury, the asteroids and the satellites, the existence or non-existence of a solar electric potential.

After the reading of this contribution, a friend drew my attention to the circumstance that I had neglected to enter into the intimate and remarkable connection existing between the periods of solar sun spots and those of terrestrial and magnetic disturbances in the form of earth currents and the Northern and Southern Lights. This connection serves actually as an important proof of the existence of electrical solar potential. If the sun spots are as I have explained to be considered as currents of solar material returned after combustion in the luminous solar atmosphere to the body of the sun, which thus becomes visible in the equatorial solar latitudes as sun spots, as the conditions are there present for the production of whirling motion, the number of sun spots must then be a measure of the energy of the combustion. The ten to eleven years' periods of the sun spots would then mean, that the combustion became more active every ten or eleven years, then gradually returning to its original amount. But as with the increase of the combustive activity a greater outflow of electrified material from the sun, and indirectly an increase of the electric solar potential must take place, the connection of sun spots with

the appearance of the northern and southern lights, earth currents and magnetic disturbances is explained. The reason of the ten to eleven years' solar period can only be sought in disturbances or partial revolutions of the gaseous solar body itself. Indeed, such revolutions must necessarily happen from time to time as the upper layers of the sun's body become cooled by the radiation of heat and light. This cooling is adjusted by the descending currents of products of combustion, which penetrate to a considerable depth into the still dissociated gaseous mass of the sun, and through admixture with it partly restore the heat lost by dissociation. The consequence will be that the outer solar layers are cooled below the adiabatic temperature appertaining to them, that therefore in place of the indifferent a stable equilibrium arises which will be maintained for some time in the same way as superheated air above ground strongly heated by solar radiation. But if this disturbance of the adiabatic equilibrium exceeds a certain limit, a change in the sun's mass must occur. The relatively colder outer layers of the sun must sink into the depths of the sun, and the relatively lighter and hotter deeper layers must take their place. Experience proves that such a change occurs every ten to eleven years. With it there is again obtained uncooled heated solar matter for combustion; this is consequently strengthened and followed by an increase of the visible rotatory back currents, the sun spots, and indirect oscillations in the magnitude of the solar potential.

I must remark in conclusion, that many of the views explained in my paper have already been published elsewhere, and I do not claim them as mine. I refer here specially to the writings of Ritter on the constitution of gaseous bodies, and Rye's book on cyclones. But it was impossible for me in the limited space of this communication to mention the authors of all the opinions which I have made use of.

A CONTRIBUTION TO THE THEORY OF MAGNETISM.*

EVER since the proper construction of electro-magnetic machines has become a matter of considerable practical importance, the question how size and shape of electro magnets may be selected, so that the greatest effect may be produced with a minimum of material and bulk, has arisen in the most various forms. The ingenious theories proposed and elaborated with a very considerable expenditure of mathematical knowledge and skill seldom supply the necessary data for the solution of these questions. No doubt the reason of this is, that the production and distribution of magnetism in magnetic bodies, of which practically iron only in its various molecular conditions has to be considered, the induction of the existing magnetism, the strength of the magnetic field depending on it, and finally, the reaction of the latter on the strength of the magnetism generated in the iron, and its distribution, have as a rule, been separately considered and calculated. Although we thus obtain the bases for solving the several questions proposed, yet a perplexing number of laws and empirical formulæ are placed before the electrical engineer, which makes it impossible for him to form a clear conception of the primary connection of the phenomena which would serve him as a guide in his construction. This unsatisfactory condition may arise from the fact that all magnetic theories started from permanent magnetism in the same way as electrical theories were founded on electrostatic phenomena being those first known. Permanent magnetism is however only a secondary magnetic phenomenon. It is the remains of a previous stronger magnetization whose laws are to be derived from those of electro-magnetism, for magnetism in general is only to be conceived of as an electrical phenomenon. The electric current, or more generally, electricity in motion is the only known source of all magnetism. I have already expressed the opinion on another occasion in this place, that this must also be the case with the earth's magnetism, and accounted for it in that till now at least no other cause is conceivable than the rota-

* Monthly Report of the Berlin Academy of Sciences, 23rd Oct., 1884.

tion with the earth about its axis of the accumulated electricity resting on its surface. Magnetic iron ore and other bodies existing in nature in the magnetic condition evidently owe their magnetism to that of the earth, or probably in special cases to the direct action of electrical discharges.

If on the other hand we proceed upon the assumption of a body directly or indirectly magnetisable by an electric current, which retains no magnetism when the cause of magnetization ceases, and assume with Faraday that the propagation of the magnetic action, both in the magnetic bodies themselves, and in the surrounding space takes place only from molecule to molecule, or from one element of space to another, then it must be further assumed, that both actions, the inner and outer, must be entirely dependent on one another. In a bar of iron around which an electric current circulates only so much magnetism can be produced by the electrical inductive force acting on it, as is bound up in the space surrounding the iron bar by the magnetic induction—proceeding in the direction of Faraday's lines of force from the north magnetic to the south magnetic particles of the bar—and thus forming a magnetic closed circuit.

If this idea is proved by experiment to be admissible, the laws of the molecular communication of heat, electricity and electrostatic induction must be also applicable with the necessary modifications to magnetism. We should then be able to lay down a universal law for the strength of magnetism of the form "Sum of the magnetising forces divided by the sum of the resistances opposed to them" which would remove many difficulties and apparent contradictions.

The further law would then also hold good that in every section, which cuts through all existing lines of force, the sum of the magnetic moments of all the magnetic molecules cut through is equal to zero.

Such a section can only pass through the neutral magnetic centre of the magnetized body, separating the north and south magnetism, and then the sum of the magnetic moments of the iron molecules cut through must be as great as that of the molecules or space elements cut through outside the iron.

The order of electrical phenomena would then be, that an electrical difference of potential making its appearance between

two bodies located in the insulating medium excites on their surfaces a quantity of static electricity of opposite polarities, whose magnitude depends on the resistance which the non-conductive surrounding medium opposes to the electrical induction. This resistance is dependent on the dimensions of the space and on a coefficient of induction proper to the material filling it. If the surrounding space is not insulating, but entirely or partly a conductor of electricity, then electric currents are produced whose strength depends on the sum of the resistances opposed to the movement of the electricity. The electric current or electricity in motion has again the property of attracting similarly directed currents and their conductors respectively and of repelling dissimilar. If one assumes with Ampère, that the magnetic material is filled with pre-existing molecular currents, then the electric current must have the tendency to turn these elementary solenoids from their position of equilibrium in such a way that their axes fall upon the peripheries of circles surrounding concentrically the current carrier. If any material, such as iron for instance, contains a greater number of such molecular currents in a unit of space, then the work of the current must be greater, for a greater number of solenoids, which the current tends to turn, is contained in each cross-section of the concentric ring. But as the intensifying effect, which the consecutive sections must exert on each other on account of the diminished distance of the elementary solenoids from one another is now also greater, then on both accounts the sum of the moments of a concentric ring of iron must be greater than that of a ring of equal dimensions filled with a less magnetic material. Another way of expressing it is that iron and other so-called magnetic bodies oppose less resistance to magnetic polarization than non-magnetic bodies, or that their magnetic conductivity is greater. Magnetic induction cannot make its appearance with rings of homogeneous material, which surround a conductor concentrically because all the lines of force lie within the ring. The relation is different in an iron ring which is not closed. Since the resistance of iron to magnetic induction is only about $\frac{1}{800}$ th that of air, as follows from the experiments described later on, the total magnetism of an interrupted ring must be less corresponding to the great increase in resistance to induction caused by the air-gap at the point of interruption, and

the lines of induction or of force uniting the portions of the ring must fill the whole surrounding space in very different strengths and produce in it the phenomenon of magnetic attraction and induction, or those of so-called free magnetism.

According to this, Ampère's theory would have to be extended, by supposing that not only magnetic bodies, but that all bodies, as well as empty space, were filled with pre-existing current whirls of very small dimensions, and that magnetic bodies only differed from non-magnetic, in that in the former the number of current whirls in a unit of space is much greater than in the latter.

All magnetic phenomena would then be referred to the property of the electric current of exerting a directive force on the molecular solenoids, distributed throughout space, and existing in greater number in so-called magnetic bodies, which seeks to place their axes at right angles to its direction, and tends to arrange them in closed concentric circles of attraction. The amount of this axial rotation depends on the one hand on the strength of the directive or magnetizing force, and on the other on the number of current whirls pre-existing in the unit of volume, for which numerical ratio the expression "magnetic conductivity" or its reciprocal "magnetic resistance" may be employed.

As a test of the admissibility of this view, the case of an iron ring or iron tube closed on itself and symmetrically wound with insulated wire, which case has been frequently examined both theoretically and experimentally, seemed to be especially suitable, for according to Kirchhoff's experiments, no magnetic induction takes place, with such a ring when symmetrically wound. I have previously obtained * from Ampère's formula the value

$$M = 4 \pi \cdot l \cdot s \cdot i.$$

for the magnetism of an iron tube, the thickness of the metal of which is s , and the length l , traversed by an axial current of the strength i , and I have proved its accuracy by experiment.

If an iron ring of section q and mean radius ρ is surrounded with a close wound coil, then according to the above considerations, the magnetizing force is proportional to the current

* Berlin Acad. Report of 23 June, 1881, p. 701.

strength i , multiplied by the number of convolutions, for which the length of the ring, *i.e.*, $2 \rho \pi$ can be approximately substituted.

The resistance opposed to this magnetizing force is directly proportional to the length of the bent iron bar, and therefore again equal to $2 \pi \rho$, and inversely proportional to the section, and magnetic conductivity of the iron which may be represented by π . Therefore the magnetic moment of the iron ring in each cross-section is

$$\frac{i \cdot 2 \pi \rho}{2 \pi \rho} = i \cdot q \cdot \psi \text{ const.}$$

$$\frac{q \psi}{q \psi}$$

which equation is equivalent to the above $M = i \cdot l \cdot s \cdot \text{const.}$

For the solution of the final question, whether the magnetism which is produced in an iron bar or in an open horse-shoe by means of a magnetizing force is also inversely proportional to the total resistance of the magnetic circuit, I had a horse-shoe prepared of bar iron 20^{mm} thick and bent twice at right angles. The legs of the horse-shoe were 70^{mm} long and each was wound with a coil of 35^{mm} length, consisting of from 126 to 130 convolutions of insulated wire 1^{mm} thick. The straight portion of the horse-shoe was provided with an induction coil of 1,160 convolutions of wire 0.2^{mm} thick. The horse-shoe could be made into a metallic closed circuit, by means of a square piece of iron of the same cross section as itself. The legs of the magnet protruded 20^{mm} beyond the coils.

The experiments were made in such a manner that the direction of the current passing through the magnetizing coils could be reversed instantaneously by means of a suitable commutator. The strength of the current before each reversal was measured by determining the difference of potential between the terminals of the magnetizing coil by means of a torsion galvanometer with very numerous coils of thin wire. By the insertion of resistances or by the application of a shunt the desired strength of current could always be obtained. The current arising in the induction coil on the reversal of the current was led through the convolutions of a nearly dead-beat mirror galvanometer. The deflection then measured double the magnetic moment excited in the magnet by means of the working current. By taking the

precaution of reversing the direction of the current several times after each alteration in its strength, before the measurement was made, concordant results were always obtained even with the greatest difference of current.

TABLE I.

Ampères.	1.		2.		3.		4.	
	Leg 90 == High.				Leg 70 == High.			
	Closed by Armature Plate.		Open.		Closed by Armature Plate.		Open.	
	Increase per $\frac{1}{100}$ Amp.		Increase per $\frac{1}{100}$ Amp.		Increase per $\frac{1}{100}$ Amp.		Increase per $\frac{1}{100}$ Amp.	
0·01	800	800	195	195	1095	1095	140	140
0·03	3150	1175	650	227	4800	1852	430	145
0·05	6250	1550	1125	237	10500	2850	750	160
0·07	10500	2125	1640	257	20500	5000	1100	175
0·09	15500	2500	2165	262	33000	6200	1410	155
0·1	18350	2850	2400	235	36200	3200	1570	160
0·15	37000	3730	3700	260	2390	164
0·2	4830	226	3100	142
0·25	6000	234	3900	160
0·3	7100	220	4700	160
0·4	9600	250	6200	160
0·5	12250	265	7900	170

The above Table gives in the first column the measured strength of current, in the first column of each succeeding series the corresponding deflections, in the second the increase of the magnetic moment calculated from it for an increase of the strength of the current by 0·01 Ampère, both with closed and open magnetic circuit. The result is that in the closed horse-shoe the magnetism increases at first in a quicker ratio than the strength of the current with weak currents (0·05 Amp.). The magnetism in the open magnet is about $\frac{1}{5}$ th of that excited in the closed magnet by equal magnetizing forces; with double strength of current (0·1 Ampère) about $\frac{1}{3}$ th. The increase of magnetism on the other hand is nearly constant with the open magnet, i.e., the magnetism was nearly proportional to the strength of the current up to the limit which could be attained without heating the coils too much.

I then had 20^{mm} of the projecting poles of the electromagnet cut off and repeated the above experiments. As follows from the

third and fourth columns of the above table the magnetism of the closed magnet increases considerably through this shortening, whilst the magnetism of the open magnet diminishes in a yet higher degree, so that now with 0.05 Ampère the above proportion is reduced to $\frac{1}{1.4}$, with 0.1 Ampère to about $\frac{1}{2.5}$. This disproportionately great reduction is evidently to be ascribed to the circumstance, that not only was the inductive resistance of the surrounding space increased by the shortening, but the magnetizing force was also diminished, as the coils had exerted a magnetizing effect also on the pieces cut off. I then sought (by screwing on to the poles lengthening pieces of equal diameter and 10^{mm} height), to determine the length of leg with which the magnetism of the open magnets doubled with constant current. This proportion was exceeded according to the following table, when 5 pieces were added, and therefore with an increase of some-

TABLE II.
Current 0.1 Amp.

	Magnetism.	Increase.
Without addition	1950	...
1. Piece on each side	2430	480
2. " " " "	2895	465
3. " " " "	3330	435
4. " " " "	3750	420
5. " " " "	4125	375

what over a half of the original magnet length. We see from the increase of magnetism which takes place with each lengthening of the magnet leg by 10^{mm}, that a somewhat considerable reduction of this increase takes place with the number of pieces added. This is also partly due to the stronger direct action of the coils on the added pieces lying nearer the coils, by which also is explained the too quick doubling of the magnetism with the increased length of the leg. But any way these experiments make it highly probable that the magnetism produced in a closed electro-magnet by a magnetizing force is a function of its surface. Finally it has also been proved by these means that the strengthening of the magnetism by the application of thin iron tubes was as great as when solid iron cylinders of the same diameter and height were

employed. The closing of the tubes with an iron cover made no appreciable difference if no increase in the length of the tube was thereby brought about.

To determine the resistance opposed to the production of magnetism in the iron by the non-magnetic surrounding space it was necessary to compare the magnetic resistance of an air-filled or vacuous space with that of iron. This ratio cannot be constant, for the specific magnetic resistance of iron varies with the strength of its magnetization.

As is known, and as may also be seen from the above experiments, the magnetism in a closed electromagnet increases at first more rapidly than the strength of the current. The increase of magnetism then soon attains a maximum, and with a further increase in the strength of the current sinks down slowly to a

TABLE III.

Strength of Current	Deflection.	Increase per 1000 Amp.	Deflection.	Increase per 1000 Amp.	Strength of Current	Deflection.	Increase per 1000 Amp.	Deflection.	Increase per 1000 Amp.
0.001	3.2	3.2	3.5	3.5	0.100	1810	65	430	5.0
0.002	7	3.25	7.2	3.7	0.15	4520	54	760	6.6
0.004	15	4	15	3.4	0.2	6880	47.2	1120	7.2
0.008	36	5.2	29	3.5	0.25	8640	35.2	1640	10.4
0.01	46	5	35	3.0	0.3	9900	25.2	2500	17.2
0.02	114	6.8	72	3.7	0.4	11500	16	4950	24.5
0.03	196	8.1	112	4.0	0.5	12400	9	7000	30.5
0.04	300	10.5	155	4.3	0.6	13150	7.5	8750	17.5
0.05	410	11	195	4.0	0.7	13750	6	10000	12.5
0.06	550	14	245	5.0	0.8	14250	5	11000	10.0
0.07	710	16	290	4.5	0.9	14600	3.5	11900	9.0
0.08	895	18.5	340	5.0	1.00	15000	4	12550	6.5
0.085	1015	24	360	4.0	1.1	15250	2.5	13150	6.0
0.090	1160	29	380	4.0	1.2	15500	2.5	13600	4.5
0.095	405	5.0	1.5	16150	2.1	15000	4.6

very slight amount. In this behaviour of magnetic bodies the initial increasing action of the magnetizing force to a maximum is very remarkable. The position of this maximum depends on the quality of the iron. The maximum, with equal increase of the magnetizing force, occurs earlier with soft iron than with hard. It is therefore not unlikely that this initial weaker action of the

magnetizing force is generally only a consequence of the unusual softness of the iron. To examine into this more closely, I had two similar rings prepared, one of the very softest iron rod and the other of soft steel. They were of 50^{mm} external and 35^{mm} internal diameter and each was similarly wound with two coils, the lower of 350 convolutions of wire 0·2^{mm} thick, the upper of 190 convolutions of wire 0·75^{mm} thick. The second coil served as a magnetizing, the first as an induction coil. The first column of Table III. gives the measured strengths of current, the second the corresponding magnetic moment of the iron ring measured by the deflection of the mirror galvanometer, the third the calculated increase in the magnetism with an increase in the strength of the current of 0·001 Ampère. The fourth and fifth columns give the same value for the steel ring. It follows from these experiments that with the iron ring the maximum of the increase of magnetism had already been obtained with 0·1 Ampère, whilst with the soft steel ring it only took place at 0·5 Ampère, and further that this last maximum was only about one half of the maximum of increase with the iron ring. As the direction of the current was reversed with each measurement, the residual magnetism could exert no direct influence on the result of the measurement, but probably the internal friction, which opposes the rotation of the hypothetical currents, must bring about a reduction of the magnetization which would be less with the ring of soft steel than with the iron one. According to this it appears at all events probable that with absolutely soft iron the maximum action would take place even with the weakest currents. We may therefore consider this anomaly which occurs in the magnetization of iron as a consequence of the frictional resistance which opposes the rotation of Ampère's current whirls; this resistance must be so much the more perceptible, the smaller the angles of rotation, for the work of friction must be proportional to the angle of rotation itself, and not to the magnetic moment excited by the rotation.

The following Table IV. gives the results obtained, when in place of a closed ring a straight iron bar of equal thickness and length was magnetized with the current increasing in strength. The iron bar was provided in the middle with an induction coil, and was pushed with this into the middle of a magnetizing coil of nearly double the length.

TABLE IV.

Strength of Current.	Deflection.	Increase per 1000 Amp.	Strength of Current.	Deflection.	Increase per 1000 Amp.
0.001	12	12	0.08	1160	16
0.002	22	10	0.09	1320	16
0.004	44	11	0.10	1480	16
0.008	88	11	0.20	2900	14.2
0.01	109	10.5	0.30	4600	17
0.02	233	12.4	0.40	6240	16.4
0.03	365	13.2	0.50	8000	17.6
0.04	524	15.9	0.60	9720	17.2
0.05	688	16.4	0.70	11560	18.4
0.06	844	15.6	0.80	13200	16.4
0.07	1000	15.6	1.00	16800	18

The direct action of the coils on one another is allowed for in the deflections of the mirror galvanometer. The increase of magnetism calculated for 0.001 Ampère shows here also a slight increase with increasing strength of current. The maximum of increase could not be reached without unduly heating the magnetizing coil.

If the assumption be correct, that non-magnetic material is filled with pre-existing molecular current whirls in the same way as magnetic, then it must be assumed that with it as with magnetic material a maximum of magnetism exists. An approach to a magnetic maximum must then be observable with very great magnetic moments of a magnetic field just as with iron. To test this I placed two square pieces of iron on the poles of the horse-shoe magnet described, which could be made to approach within a slight distance of one another. The parallel opposed surfaces were reduced to a square centimetre. The increase in the magnetic moment of the magnetic circuit was now measured for increasing strength of current. The results are collected in the following Table.

The pole pieces were successively removed 0.1, 1 and 3.5^{mm} from one another. The current strengths were increased to the highest permissible value, that is up to 6 Ampères. As seen from column 1, with a short-circuited magnet the maximum increase of magnetism was already reached with 0.2 Ampère. It is remarkable that when the pole pieces were applied and approached to one another, the turning point for each distance apart took place at

TABLE V.

Intensity of Current.	Horse Shoe closed by Iron Plate.		Pole Pieces supplied with Prism shaped Pieces. Distance of latter.					
	Deflection.	Increase per 1/100 Amp.	Deflection.	0.1 mm Increase per 1/100 Amp.	Deflection.	1 mm Increase per 1/100 Amp.	Deflection.	3.5 mm Increase per 1/100 Amp.
	1		2		3		4	
0.11	219	19.9	80	7.2	49	4.4	37	3.4
0.21	720	50.1	193	11.3	107	5.8	78	4.1
0.53	1708	30.9	620	13.3	313	6.4	217	4.3
0.85	2124	13.0	1020	12.5	524	6.6	362	4.5
1.06	2292	8.0	1276	12.2	680	7.4	460	4.7
2.12	2640	3.3	2028	7.1	1344	6.3	942	4.5
3.18	2760	1.1	2400	3.5	1908	5.3	1380	4.1
4.24	2840	0.7	2620	2.1	2340	4.1	1820	4.2
5.30	2870	0.5	2728	1.0	2576	2.2	2172	3.3
6.36	2930	0.4	2800	0.7	2700	1.2	2440	2.5

the same current strength of about 1 Ampère. The increases themselves could not be in proportion to the distances apart of the parallel iron faces, as the side faces of the additional pieces must take all the more part in the magnetic turning action, the further the parallel faces were separated from one another, and consequently the greater the magnetic resistance existing between them. That a maximum increase takes place at all distances apart, and that with increasing strength of current this uniformly diminishes to a small amount, is a consequence of the great moment of the electromagnet itself, which owing to the small magnetic resistance of the whole circuit with strong currents soon reaches its maximum magnetization.

The experiments appear to indicate, it is true, an increase in the inductive resistance of non-magnetic material with very high magnetic moments, but they are not decisive. For such experiments electromagnets must be employed of slight length and of cross-section so large that the inductive resistance of the iron may always be very small in comparison with that of the surrounding medium.

But the experiments show that a maximum of magnetization cannot occur earlier with atmospheric air than with iron. It

hence follows that the strength of a magnetic field is only limited by the maximum magnetism of the iron, and that the magnetic conductivity of the non-magnetic material has to be considered as constant. As this value is very variable for iron, and as no law has been found for its dependence on the existing magnetic moment of the mass of iron, a comparison of their magnetic conductivity is in general out of the question. But as in the construction of magnetic machines it is almost always that magnetic condition of the iron for which the increase of magnetism with increasing magnetizing force is a maximum, which is of special importance, this condition can be made use of as the basis of comparison.

To ascertain this proportion I had two square iron plates made 4^{mm} thick and 80^{mm} square, which could be screwed sideways to the square-shaped movable pieces added to the poles of the previously described horse-shoe magnet. At a distance of 5^{mm} from one another, with a current strength of 0.1 Ampere, the plates gave the same magnetic increase, as an iron plate of 1 sq. mm. section, which, on screwing off the plates, connected the pole pieces which were 5^{mm} distance apart. The iron preponderated with weaker magnetizing forces, the plates with stronger. This gives the magnetic conductivity of iron in its condition of greatest permeability as 480 to 500, when that of air is taken at 1. The experiment was repeated with the plates at greater distances apart, with iron wire, sheet iron and square iron rods, and the same numerical ratio was obtained.

The question yet remained, whether this inductive resistance of air of about 500 times that of iron is not to be ascribed partly to the influence of the magnetic oxygen existing in the air. To investigate this I had two round iron plates of 8^{mm} diameter connected together by means of a soldered brass ring. It was possible to fill the space between the iron plates placed at a distance of 5^{mm} apart with any desired gas, or to exhaust it by means of tubes which could be closed by cocks, fitted into two openings in the brass ring. The iron plates so connected were then fastened to the extending pieces of the polar extremities of the electromagnets, and the magnetic moment of the magnetic circuit measured with different strengths of current. Not the least difference could be observed whether the space between the plates was filled with

atmospheric air, oxygen or hydrogen, or whether it was exhausted as far as possible by means of a mercury pump.

It hence follows that the magnetic property of oxygen and the influence in general of matter other than iron and other so-called magnetic metals on the magnetization only becomes of influence with very great magnetic moments, such as those with which diamagnetic phenomena appear, and that for non-magnetic bodies only relations of space need be taken into account in magnetic phenomena. Whether this will not lead us to replace Ampère's molecular current whirls in accordance with Father Secchi's and Edlund's views by æther vortices which fill all space, and which are only present in magnetic bodies in much greater number or strength, may here remain undiscussed. It would anyhow explain the remarkable facts, that magnetic induction and attraction occur in a vacuum just as in non-magnetic material.

It follows from the following experiment that space filled with non-magnetic material as well as a vacuum is affected by electric currents qualitatively just as iron in its condition of greatest permeability, only 500 times less so.

I had 2 coils 87^{mm} inside diameter and 100^{mm} long, made with convolutions of insulated wire 1^{mm} thick, and placed them with their axes parallel at a distance of 131^{mm} apart. The two extremities of these solenoids lying near to one another were respectively provided with an iron plate on which was wound between the solenoids an induction coil. The iron plates were firmly clamped to the coils by thin brass bolts passing through their axes. The two solenoids and the two iron plates thus formed a closed horseshoe magnet, whose magnetic moment was to be measured by the induction coils on the iron plates. The results are tabulated in the following table. It is evident from the table that all the phenomena are just the same as though the iron plates had been connected together by iron cylinders instead of brass bolts. When I replaced the latter by iron cylinders 4^{mm} thick, which were therefore about $\frac{1}{800}$ th the cross section of the solenoids* with a current strength of 0.20 Ampère, the magnetic moment was in fact nearly double as strong as before, as the sixth column of the table shows,

* If the diameter of the effective air cylinder is measured to the middle of the convolutions, then the sections are as $\frac{\text{iron}}{\text{air}} = \frac{1}{560}$.

TABLE VI.

Intensity.	Solenoid alone.		As Open Horse Shoe.		As Closed Horse Shoe.		Closed with Iron Core.		Ratio of Deflection of Two Last.
	Deflection.	Increase.	Deflection.	Increase.	Deflection.	Increase.	Deflection.	Increase.	
0·01	51	51	55	55	65	65	1·18
0·05	270	55	295	61	380	79	1·27
0·10	52	5·2	580	62	640	68	1000	124	1·56
0·15	900	64	1020	76	1920	184	1·88
0·20	1236	67	1392	74	2864	189	2·06
0·30	1928	78	2160	77	4480	162	2·07
0·40	2616	69	2960	80	5850	137	1·97
0·50	258	5·2	3360	76	3800	84	7200	135	1·89
0·75	5250	77	6075	91	10250	122	1·68
1·00	7240	78	8400	93	12880	105	1·53

which gives the quotients of the deflections with and without iron cylinders. That with very weak currents the quotients were not very different from 1, then increased rapidly to double this value, and then slowly diminished, is to be ascribed to the property of the iron connection of this air magnet, and the iron cylinder of opposing a greater resistance to magnetization both with very weak and very strong magnetic moments.

The following observation will serve as a direct proof of the correctness of the assumption that in an iron bar around which electric currents circulate, only as much magnetism is excited as is bound by the sum of the magnetic moments of the air or space in contact with its surface.

If an iron cylinder of radius r , regarded as infinitely long, is surrounded somewhere sufficiently far from its ends by a coil of wire, and if y represents the magnetic moment which a current passing through this coil gives to the unit cross section at a given distance x from the middle of the coil, then the magnetic moment of this cross section is $r^2 \cdot \pi \cdot y$. This magnetic moment must become smaller as x increases, and if the theory set up is correct, by just as much as is bound by the moment of the film of air in contact with the surface of the lengthening piece $d x$.

There hence arises the differential equation—

$$-r^2 \pi \cdot dy = 2 r \cdot \pi \cdot dx \cdot y, \quad -\frac{dy}{y} = \frac{2}{r} \cdot dx$$

$$-\log \frac{y}{y'} = \frac{2}{r} x \quad \left| \begin{array}{l} x \\ x-c \end{array} \right.$$

if c represents the interval for which integration is to be performed, or

$$\log e^{\frac{y}{y'}} = \frac{2}{r} \cdot c$$

and for equal displacement, with rods of different diameter $2r$ and 2ρ :

$$\log e^{\frac{y}{y'}} : \log e^{\frac{z}{z'}} = \rho : r.$$

These equations prove firstly that for the same iron cylinder the quotient of the magnetic moments of two cross sections equally distant from each other is constant over the whole half cylinder, that therefore also equal displacements of a test-coil must always result in equal per-centage reductions of the magnetic moment. They show further that with rods of different thickness and for equal displacements of the test coil the logarithms of the quotients of the moments vary inversely as the diameter of the rods.

However, in setting up the differential equation the assumption is made that the moment of the layer of air touching the surface of the rod depends only on the moment of the unit section of the rod at the respective place. That is to say that the inductive resistance of all the external magnetic circuits was the same. But in fact the inductive action takes place between each element of the surface of one half of the rod, and all oppositely magnetic points of the other half. It is therefore also dependent on the distance from the middle of the rod. This source of error will have all the more influence the nearer the displacement through the length c is to the middle of the rod.

The above experiments confirm the assumption that there is no free but only confined magnetism, and that a magnetic force can only produce as much magnetism in magnetic bodies as is confined in them and in the surrounding medium by magnetic induction in the form of closed attraction curves with equal magnetic moment in each cross section.

This representation is quite analogous to that of electric molecular induction, and hence the laws which hold good for this may be applied to magnetic induction, and with the help of the ascertained coefficient 480, which expresses the ratio of the magnetic resistance of air to that of iron, the influence of the mass and form of the

TABLE VII.

Distance between the Centres of the Coils. mm	ϕ 9 mm.		ϕ 6 mm.		ϕ 3 mm.	
	Deflection.	$\frac{y}{y_1}$	Deflection.	$\frac{y}{y_1}$	Deflection.	$\frac{y}{y_1}$
	y		y		y	
90	4268	0.032	4654	0.057	290	0.112
100	3960	0.037	3558	0.052	222	0.116
110	3640	0.035	3160	0.052	168	0.121
120	3360	0.035	2800	0.059	130	0.111
130	3100	0.038	2440	0.053	100	0.114
140	2840	0.032	2160	0.056	80	0.097
150	2640	0.038	1900	0.048	60	0.125
160	2420	0.037	1700	0.058
170	2220	0.028	1488	0.052
180	2080	0.039	1320	0.056
190	1900	0.033	1160	0.058
200	1760	0.040	1016	0.060
210	1605	0.040	884	0.059
220	1465	0.032	772	0.052
230	1360	0.033	684	0.051
240	1260	0.036	608	0.061
Average		0.035	...	0.055	...	0.114
Average \times thick- ness of rod . . . }		0.105	...	0.110	...	0.114

iron on the strength of a magnetic field to be employed can be determined.

If an iron bar on the centre of which a magnetizing force acts may not be considered as of infinite length, the formula calculated for the bar of infinite length—

$$\log \frac{y}{y'} = \frac{2}{r} x,$$

is not directly applicable on account of the magnetism of the end surfaces.

The distribution of the magnetism is quite different in a bar of limited length, if the magnetizing force is allowed to act equally on all parts of the bar. The decrease in the magnetic moment from the middle of the bar to the ends then loses its logarithmic character, and assumes, as Van Rees has already pointed out, the form of the catenary or approximately that of a parabola. With a rod 150^{mm} long and 7.7^{mm} in diameter, which was movable in a closely surrounding glass tube of nearly double its length wound round

symmetrically with a magnetizing coil, the magnetic moment of each cross section could be measured by means of an induction coil wound over the middle of the glass tube, when the direction of the magnetizing current was reversed.

In the following table the magnetic moments are given for the same rod at distances of 20 to 70^{mm} from the centre, when the magnetization took place uniformly and from the centre. The current strengths were so chosen that the magnetism at the ends of the rod was nearly the same in both cases.

Distance x of the Secondary Coil from the Centre of the Rod.	Deflection y with uniform Magnetization.	Deflection with Magnetization from the Centre.
—	—	—
20	287	463
30	263	378
40	233	302
50	195	229
60	145	160
70	87	92

Calculated from the equation of the parabola

$$\frac{x^2}{y} = 2p$$

the deflections of the second coil give for uniform magnetization of the rod, the values

$$2p = 23 \cdot 22 \cdot 22 \cdot 23 \cdot 22.$$

If the quotients $\frac{y}{y_1}$ are calculated according to the formula

$$\log \frac{y}{y_1} = \frac{2}{r} x$$

from the values of the third column, for a constant displacement of the induction coil by 10^{mm}, the values 1.25, 1.25, 1.32, 1.4, 1.4, are obtained. The ratio of the magnetic moments of points of the rod equidistant from each other is therefore not constant, as with rods of unlimited length, but increases as we approach the end of the rod, as was to be expected.

Van Rees has found that in a homogeneous prism-shaped magnet, the magnetic moments of the sections diminish parabolically as with a uniformly magnetized iron bar. But this would only be the case with magnets to which in magnetizing an equal moment was given over the whole length of the bar. With a cessation of the magnetizing force all the molecular magnets have the same tendency to return to the non-magnetic condition, whence finally the same position of magnetic equilibrium must result as exists with uniformly magnetized iron rods.

Finally I should like to add a few remarks on a previous communication delivered by me in this place.

I then proposed the theory that the molecular magnets assumed in the Ampère-Weber theory must each consist of two elementary magnets or solenoids near one another, capable of free rotation together in every direction without resistance, but directed by external magnetizing forces and turned round one another in the same way as would be the case with pairs of astatic needles moving freely except as regards the distance of the pivots from one another. I did not then know that Stephan had already expressed the same view, accompanied by important mathematical considerations. According to my views explained above, Ampère's theory must be extended as already mentioned, by supposing all space to be full of paired molecular solenoids, or æther whirls, if one adopts Edlund's views that the electric current is æther in motion through space, and that these are found in greater number in magnetic than in non-magnetic matter. As now a magnetizing force acting upon the molecular magnets only exerts a perceptible influence on the separate rotation of the paired elementary magnets when all the neighbouring ones in the magnetic circle follow the motion and so can make a closed system of equilibrium subject to mutual attraction, it follows that the rotation directly produced by the magnetizing force must be very small compared with the mutual strengthening of the rotation in the closed magnetic circuit. The magnetic moment generated must therefore be essentially the product of the mutual strengthening of the rotation which the magnetizing force induces. But here we meet with the difficulty that the rotation becomes nil, with the absence of coercive force after the cessation of the magnetizing force. Such a condition of equilibrium can only be imagined as produced through a

simultaneous action of attracting and repelling forces. There must then be produced by the mutual action of all neighbouring attracting and repelling molecular forces a nearly but not quite unstable equilibrium of the elementary magnets if the assumptions of Ampère's theory are to correspond to facts. I do not venture to assert that a combination of molecular forces fulfilling this necessary claim can be shown to be possible.

ON A CONTRIVANCE FOR REPRODUCING THE UNIT
OF LIGHT PROPOSED BY THE PARIS CONFERENCE
FOR THE DETERMINATION OF ELECTRIC
UNITS.*

THE international conference for the determination of electrical units which met lately in Paris, has determined amongst other units to employ as unit of light the quantity of light which is radiated from a surface of a square centimetre of melted platinum when just solidifying. If this unit is to have a practical application it is necessary to find out a simple and easily available contrivance for making comparative measurements with it at any desired place. This problem was apparently made much more difficult by the circumstance that platinum on fusing readily takes up carbon, silicon, and other materials by which its temperature of fusion is considerably lowered.

The little apparatus which I now bring before the Academy does not correspond exactly to the definition of the unit of light, as fixed at Paris, as it furnishes the quantity of light given by melting and not that given by solidifying platinum. It is however probable that with chemically pure platinum this difference is very small. The contrivance is based on melting a very thin sheet of platinum by the galvanic current. The platinum foil is enclosed in a metal case which permits the light of the glowing sheet to pass out only by a slot of 0.1 square centimetre section which is

* Poggendorff's *Annalen d. Phys. u. Chem.*, 1884, Vol. XXII.

nearly opposite to it. The walls of the slot are conical and the platinum sheet overlaps it on all sides. At the moment of melting the platinum sheet there passes from the slot a quantity of light of 0.1 of the unit of light.

By slowly increasing the strength of the current it can be arranged that the melting of the plate takes place only at the moment when the light is exactly balanced by that of the source of light to be measured. According to preliminary measurements the quantity of light which passes out from the apparatus at the moment of melting is nearly equal to 1.5 of an English standard candle.

ON THE UNITS OF ELECTRICITY AND LIGHT ACCORDING TO THE RESOLUTIONS OF THE PARIS INTERNATIONAL CONFERENCE.*

DR. NEESEN and Von Helmholtz have in due course fully reported to the Society the resolutions of the international electrical congress which was held in Paris in 1881. As is known, a conference of delegates from different States was held in Paris in the year 1882, on the basis of the resolutions of the congress, for the definite settlement of the units. As this conference was convinced that the experts had not yet obtained sufficiently accordant results to be able to fix numerical values for the units, they adjourned giving a final decision until an agreement in the determinations of the ratio of the ohm to the mercury unit had been obtained to within $\frac{1}{100000}$ th. In the course of this month a second conference of delegates has taken place in Paris, which has brought the work to a provisional termination. As Mr. von Helmholtz, who unfortunately can only appear later, intends to give us a communication on the labours of both conferences, I confine myself to a few considerations on the final decisions of the last conference, and combine with it a proposal for practically carrying out the definition of the unit of light proposed by the conference.

* Address at the meeting of the Electro-technical Society of the 2 1884.

As the new determinations of the ohm made by different observers had not agreed to within $\frac{1}{1000}$ th of its value, as considered necessary by the former conference, the conference resolved to fix its value in the meantime at 1.06 of the mercury unit, leaving it to subsequent more exact researches to add further decimals as soon as they are acknowledged to be absolutely established. Until the international recognition of such a more exact determination, 1.06 of a mercury unit is the legal unit of resistance, the "legal ohm." In the same way as regards the unit of resistance, the congress, and following it the final conference, has given special names to the units of electric potential, current strength, quantity of electricity, electrostatic capacity, viz., Volt, Ampère, Coulomb, Farad. Unfortunately we Germans miss among these names of celebrated electricians that of the author of the absolute system of measurement itself, our Wilhelm Weber. It was indeed really an adverse concatenation of circumstances which brought this about. Speaking strictly, these denominations are moreover only of importance during the period of transition, whilst there is a fear without further definition of confounding these with other units hitherto employed. If the C. G. S. system is once legally established and everywhere in practice a special nomenclature for the units is quite superfluous. The simple numerical data will then be sufficient, for it is self-evident that strength of current cannot for instance be measured with units of resistance or capacity. Only when it is desired to make use of a unit unsymmetrical with the adopted C. G. S. system, it must be specially denoted. If, for instance, the mercury unit is used, the sign Hg should be put after the numbers representing the measured resistance, in order to show thereby that the mercury and not the legal unit is meant.

Although the final conference has declared its work completed, after it has fixed the value of the legal unit of resistance in mercury units, and has given an exact definition of the remaining electrical units and of the unit of light, the subject is still far from being complete. The standardizing of these different units and the discovery of practical methods for their reproduction still demand considerable and in a degree very difficult scientific work. It is to be hoped that this important work will be very zealously prosecuted, so that the science and practice of electricity

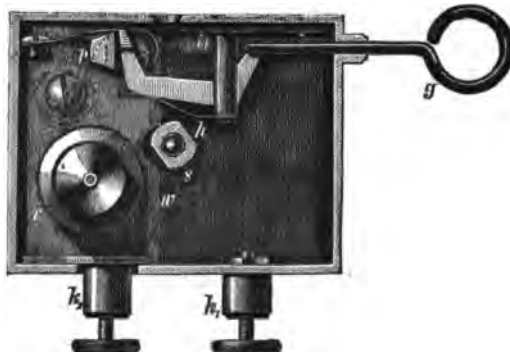
may soon derive benefit from a system of measurement which has been carefully prepared and is convenient in application.

Fig. 40.



The conference has adopted as the unit of white light that quantity of light which is radiated from a square centimetre of

Fig. 41.



pure melted platinum at the temperature of solidification ; as the unit of coloured light the quantity of similarly coloured light which is contained in this white light. This definition of the

unit of light caused great hesitation in the circle of the conference itself. It was objected that the light radiating from melting platinum could not be considered white in comparison with sun light and electric light; that up till now there was no certain method of absolutely producing the unit of coloured light as defined from the quantity of white light taken as unit, in a manner suitable for measurement; that the means of melting platinum without contaminating it with carbon, silicon, or other bodies, were very imperfect and difficult to carry out in practice; and that finally it was difficult to carry out exact photometric measurements with a melted platinum mass. On these grounds I proposed at the conference to employ provisionally as a practically applicable unit of light the small standard lamp which von Hefner Alteneck constructed and has brought before the meeting of the Society. If this lamp has still many defects, amongst which I may mention that it gives a somewhat coloured light, and like all flame lights is exposed to many disturbances and requires many corrections, yet it gives very certain results in comparison with the standards of light hitherto employed, is very convenient in use, and like the mercury unit could serve as a point of departure and interim unit until the problem of a rational unit of light has been solved. On the English side it was proposed to employ as unit the quantity of light radiated from a carbon filament (Swan lamp) by a determined quantity of electrical work. None of these proposals however met with the approval of the conference, for von Hefner's lamp was opposed for the reasons stated, and the electrical glow-lamp on account of the dependence of the quantity of light emitted by it for equal temperature of the filament on the molecular condition of the surface of the carbon filament. The platinum unit was therefore finally considered by the conference as relatively most to be depended on, and adopted as the legal unit of light.

I have now set about producing this unit in a more convenient form than was done by Monsieur Violle, who proposed it. I have succeeded in effecting this in an exceedingly simple way. Certainly the little lamp which I will show you directly does not correspond precisely to the definition given by the conference, for the light is emitted, not from molten platinum at the moment of solidification but from platinum at the moment of fusion. It is still unknown whether there is any considerable difference between the tem-

peratures of the fusion and solidification of pure platinum. If such a difference really exists the results given by my lamp must be corrected by a coefficient to be ascertained in order to give the legal unit of light. The lamp depends on the fusion of a very thin sheet of platinum 5 or 6 millimetres wide by an electric current passing through it. The platinum sheet is enclosed in a small metal case, in one of the thin walls of which there is a small opening conical towards the inside, the least section of which is almost exactly 0.1 of a square centimetre. Directly behind this slot is the platinum sheet which completely covers the edges of the former. If this sheet of platinum is brought to incandescence by the insertion of a few galvanic cells, the quantity of light emitted from the slot is exactly as great as if the seat of the emission was in the plane of the opening. If the battery is provided with an arrangement which permits of the strength of the current being very slowly increased, there is time to maintain the photometer continually in the position of equilibrium until the platinum melts and darkness suddenly appears. The light issuing from the slot the moment just before this takes place is exactly one-tenth of the unit adopted by the conference for white light. A small cutting tool in the lamp case makes it possible, by a simple back and forward motion of a handle, to place in front of the hole a new piece of platinum off a roll in place of the melted piece, so that the experiment may be repeated without loss of time as often as desired.

The method of melting the platinum here described has the advantages over melting it in a lime crucible of incomparably greater simplicity and easier handling, besides the essential superiority that the platinum sheet can be formed of pure platinum, and is not rendered impure by melting. As further the platinum sheet may be very thin—about 0.02^{mm} is thick enough—the consumption of platinum is very small.

The unit of light adopted by the conference is rendered practically applicable by means of this apparatus, and is thus in fact the most reliable and most rational measure of light which up to the present can be brought forward.

How far this apparatus can be employed with advantage for the direct measurement of light will only be shown in practical use. Probably electricians will prefer as a rule to employ glow lamps

for their measurements, which they can determine and control from time to time by means of the platinum unit of light. In the same way gas engineers will prefer as a rule to use Von Hefner's normal lamp, which has the advantage that the colour of its light is very like that of a gas flame, and that it is subject to the same weakening influences. The comparative measurements which I have had made with the platinum lamp have yet no numerical importance, because as yet no chemically pure platinum could be employed in the sheet. They show, however, that the measurements are very uniform and easy to carry out. They have given for a Von Hefner's normal lamp or a normal candle of 40^{mm} height of flame 0.07 of the unit of light adopted by the conference. It is probable, however, that the measurements will be somewhat smaller when chemically pure platinum is used.

ON THE CONSERVATION OF ENERGY IN THE EARTH'S ATMOSPHERE.*

IN my communication to the Academy "On the admissibility of the assumption of a solar electrical potential and its importance for the explanation of terrestrial phenomena,"† I sought to refer certain still mysterious meteorological phenomena to disturbances of the dynamical equilibrium of the atmosphere. Further consideration of these interesting questions has shown me that a consistent application of the fundamental law of the conservation of energy in the atmosphere leads to their explanation in a much higher degree than I had previously apprehended.

The interdependence of meteorological phenomena has been very closely studied by meteorologists during the last ten years. There exist on this subject an almost unwieldy mass of observations on which several ingenious theories have been founded. These deal, however, mostly with secondary phenomena, and rest

* From the report of the meeting of the Berlin Academy of Science, XIII., Session of 4th March, 1886.

† Meeting of 31 March, 1883.

therefore on a narrow foundation. It would appear, indeed, as if modern meteorology had somewhat neglected for these special studies the investigation of the first causes of the observed phenomena. Dove, in his theory of wind and storms, however, sought to account for them altogether in the ascending currents of air of the torrid zone, which forming a higher ring of air above it, must flow towards the poles, and explained the continual changes in the direction and strength of the winds by the collision of this equatorial current with the masses of air flowing back from the polar regions to the equator. Even although no proper basis was to be found for this contention in collisions of opposed currents of air, and considering the general uniformity of the mean pressure of the air of the whole atmosphere, it could not be correctly understood why the air should move with such energy from the polar regions to the equator so far off in comparison with the height of the atmosphere, yet this explanation is more satisfactory than the usual almost exclusive reference of the phenomena of the motion of the atmosphere of the higher latitudes to maxima and minima pressures of the air, of which no one could actually say whence they come or whither they go. These explanations of the direction and strength of the winds can only be said to have a scientific basis if it can be shown where the forces are located and have their point of application, which, often in a hardly appreciable manner, accumulate such a great energy in maxima and minima, and then in turn generate storms and cyclones.

In the following pages it will be attempted to supply these deficiencies by the light of the doctrine of the conservation of energy.

There is general agreement to the effect that all life and motion on the earth originate in solar radiation. Without a supply of heat from the sun the atmosphere would be motionless, or rather would follow the earth's rotation without any relative change of place or temperature, if the radiation from the stars and the proper heat of the earth are neglected. The rotation of the earth would give to the atmosphere the form of an ellipsoid of revolution if at the temperature of space it continued gaseous and was subject to Mariotte's law, but it could never produce a continuous circulation of the air as has often been assumed. As the mean

temperature and mean motion of the air change in any definite time as little as the rotation of the earth itself, a constant quantity of solar energy must be stored up in the earth's atmosphere in the form of sensible and latent heat, of kinetic energy of masses of air in motion, or as local accumulations of pressure. Correspondingly the supply of heat by solar and stellar radiation must be equal to the loss of heat by radiation into space. The supply of heat takes place partly by direct absorption by the atmosphere of rays traversing it, but chiefly by the heating of the earth's surface, and is mainly expended in heating the lower strata of air and in evaporation of water. The loss of heat by radiation into space also takes place chiefly from the solid and fluid surface of the earth, and only to a slight degree directly from the mass of air. There are here two important points to be noted. Whilst the solar radiation considered as emitted from one point falls chiefly on the lower latitudes, the radiation directed to all portions of space is independent of the geographical latitude and is only dependent on the difference of temperature between the radiating portions of the earth's surface and that of space. As the stellar radiation which apparently warms space, is for all parts of the earth's surface as its radiation, it may be neglected, and the temperature of space may be taken as the absolute zero. It is further to be observed as regards the radiation from the earth, that the direct loss by radiation of the higher and less dense layers of air must be greater than that of the lower layers, since radiation into a vacuum is greater than into space filled with air.

This being premised, the following conditions for the equilibrium of the atmosphere may be maintained :

1. The condition of equilibrium of the motionless atmosphere is indifferent, the corresponding curve of temperature is adiabatic ; which means that the removal of a mass of air from one level to another, setting aside loss by friction, is attended neither with gain nor loss of energy.
2. A disturbance of the indifferent equilibrium of the atmosphere, corresponding to a local accumulation of energy, is brought about by the masses of air lying nearer the earth's surface getting heated above the adiabatic temperature proper to them, as well as by the cooling through increased radiation of the higher layers of air below that temperature. The heat expended in the evapora-

tion of water increases this disturbance of equilibrium in the same direction and proportion, since steam has less specific gravity than air, and as the latent heat of the steam condensed by the adiabatic cooling of the air when rising is used for heating and expanding the air.

3. The energy accumulated in the disturbance of the indifferent equilibrium of the atmosphere through overheating of the lower and overcooling of the upper layers of the air must be equalized by up and down flowing air currents. According to Clausius' second law of the mechanical theory of heat, the excess of heat of the expanding air is transformed for the greater part into the kinetic energy of moving air ; to a less degree it is distributed to greater and relatively cooler masses of air. The accelerated ascending current of air must therefore retain a negative excess of heat up to the greatest rarefaction above the adiabatic temperature corresponding to the altitude.

4. The kinetic energy accumulated in the accelerated up and down going currents of air can only be destroyed by being again transformed into heat by internal or external friction or local increase of pressure.

5. The mechanical energy accumulated in the rotation of the atmosphere round the earth's axis must be constant, and in the relative state of rest correspond to the velocity of rotation of that portion of the earth's surface upon which it rests. As, in consequence of equatorial and polar currents of air, a continual change in the geographical position of the masses of air takes place, the velocity of rotation of the whole air must in the lower latitudes lag behind the velocity of rotation of the earth's surface, and on the contrary gain upon it in the upper latitudes. The amount of friction with the earth which reduces these differences of velocity, must be as great in the equatorial regions as in the polar latitudes, in order that the constancy of the mean velocity of rotation of the whole atmosphere may be maintained. The velocity lost by friction only affects therefore the local amount of the difference of velocity.

6. On the boundaries of the air currents of different velocities a continuous admixture of neighbouring portions of air possessed of different velocity takes place. By this process, analogous to friction, a diminution of the quicker and increase of the slower

flowing currents takes place proportional to the difference of velocity. There is thus produced in the boundaries an increase of pressure in the quicker and a reduction of pressure in the slower currents of air.

Of these fundamental propositions the two latter only require special explanation.

If we assume the whole atmosphere at relative rest and neglect its height as small in comparison with the earth's radius its kinetic energy will be

$$K = \frac{4 \tau^4 \pi^3 q}{\tau^2} \int_{-\frac{\pi}{2}}^{+\frac{\pi}{2}} \cos a \, da = \frac{16}{3} \frac{\tau^4 \pi^3 q}{\tau^2}$$

where K is the sum of the kinetic energy, q the weight of air resting on unit surface, τ the period of revolution of the earth in seconds, and a the angle of latitude. For the mean velocity of the air which corresponds to this value of the kinetic energy we have

$$c = \sqrt{\frac{2}{3}} \cdot \frac{2 \pi \cdot \tau}{\tau} = 379 \text{ metres per second.}$$

This is the velocity corresponding to the 35th degree of latitude.

If one now assumes the whole atmosphere to be suddenly intimately mixed so that each particle has assumed the above mean velocity, then the air from the equator to the 35th degree of latitude must rotate more slowly than the earth's surface, and that in the higher latitudes on the contrary more quickly. At the equator itself this difference of velocity would be 84 metres in the direction of east to west, at the 45th degree 59 metres, and at the 54th degree 107 metres in the direction from west to east. Through friction with the surface of the earth this difference of velocity would be gradually reduced if no currents of air occurred in the direction from the equator to the poles and *vice versa*. As there always are these currents, however, a condition of equilibrium must result by which the mixture of the more quickly rotating equatorial with the more slowly rotating polar air is so far brought about, that the accelerated friction of the equatorial zone up to the 35th degree of north and south latitude is equal to the retarding

friction of the rest of the earth's surface. Therefore there must be east winds in the whole atmosphere of the equatorial zone, a preponderance of west winds in the regions north and south of the 35th degree, and the prevalence of west winds must increase with the latitude.*

We must now consider the hypothetical case, that the earth is a smooth solid globe with a homogeneous surface, and that the hygroscopic state of the atmosphere is vanishingly small. Then the indifferent equilibrium and the adiabatic temperature of the strata at different altitudes of the atmosphere would only be affected by the currents of air caused by the varied heating of the air by solar radiation and their cooling by radiation outwards. The heating of the air, and especially of the lower strata, is by far greatest in the equatorial zone, and diminishes from there approximately as the cosine of the latitude. Consequently the transformation from solar energy to the kinetic energy of air in motion must be greatest at the equator and diminish on approaching the poles. This transformation takes place in the ascending currents. If we disregard for a moment the displacement of the torrid zone due to the changes of seasons the conditions exist for a general and continuous up-current of air. In fact there is in the lower trade winds a continuous flow to the equator of air coming from the regions nearer the poles. This current of air must here have a lower velocity of rotation than the surface of the earth below it, and must therefore blow from east to west for the reason already mentioned of the conservation of the mean velocity of rotation of the atmosphere. As the north and south components of both the lower trade winds, assumed of equal

* Unfortunately it is only within the last few days that I have received Dr. A. Sprung's recently published text-book, from which I have learnt that Ferrel from similar observations has specified the 35th degree of latitude as that above which all the currents of air must have a southerly direction. I cannot, however, agree with him that owing to the retarding friction of the air on the earth's surface, the position of this zone must in general be displaced towards the equator. Friction on the earth's surface can, according to my view, only diminish the difference of velocity, particularly of the lower equatorially directed currents of air, but not the place where this difference between the velocity of the earth and air is nil. The author of this very remarkable work has evidently attempted throughout to furnish a mechanico-physical basis for meteorological phenomena, and has, therefore, frequently arrived at similar views to those here given. But it unfortunately has not been possible for me to discuss further the differences existing between us in some very essential points.

strength, neutralize each other, on approaching the equator from opposite sides, their kinetic energy increases the upward motion of the air. An upward motion of the whole of the air of the torrid zone in rising spirals, opposite in direction to the earth's rotation, must therefore take place. Only over the equator itself can a ring of air remain which cannot take part in this ascending motion, up the northern and southern surfaces of which the spirally ascending trade winds slide. By carrying with them the limiting layers of the relatively quiescent equatorial masses of air, regularly diminishing whirls must be produced which communicate to the centre of the mass of air an opposite velocity, and therefore similar to that of the earth's rotation. This is the region of calms. The portions of the trade wind lying nearest to the earth's surface and therefore most strongly heated, unite above the wedge-shaped ring of calms with its narrow edge upwards, and form the central portion of the great equatorial upcurrent. The velocity with which these masses of air ascend must increase proportionately to the rarefaction of the air with upflow brought about by the reduction in pressure, because equal masses of air must pass through each horizontal section in the unit of time; and the kinetic energy thus brought about must drive the upflowing air high above the upper limits of the atmosphere, until its weight, no longer balanced by the pressure of the surrounding layers of air, has destroyed the vertical component of its velocity. Hence there is formed above the middle of the hot zone the equatorial ring of air described by Dove, obviously similar to the sun's protuberances and flames which must continually flow towards the poles.

This overflowing is produced by the accelerating pressure of the air masses driven by the excess of velocity gained in the ascent beyond the pressure equilibrium; the velocity which is thus communicated to the highly rarefied masses of air flowing towards the poles, must therefore be equivalent to the maximum velocity gained in the ascent. But it can only be the middle layers nearest the equator, of the extensive region of the equatorial upflow, which can maintain the vertical direction so as to destroy the vertical component of their kinetic energy by gravitation. This follows at once from the consideration that everywhere in the earth's atmosphere the masses of air flowing towards the poles and

to the equator must be equal for each latitude unless local differences of pressure occur. The paths of the masses of air rising with accelerated velocity in the torrid zone must therefore be deflected all the sooner to the poles, the greater their distance from the equator. If we trace the paths of these different currents of air we see that the strata of air lying nearest the earth of the masses of air flowing to the equator—which are also most heated by solar radiation in the neighbourhood of the equator—flow up in vertical paths to the greatest height, and are driven thence with the greatest velocity to the poles; and that the higher strata of the trade winds do not reach the greatest heights of the atmosphere, and are driven so much sooner in the polar direction from the equator the greater their distance from it, and the greater at the same time their original height above the surface of the earth.

The course of the air-currents in the torrid zone will then be: the lower trade wind retarded by friction with the earth's surface increases in velocity with its height above the ground; then we have at an unknown height a space between the upper and lower trade-winds filled with horizontal cyclones. Above this the current directed to the poles prevails up to the greatest atmospheric heights, and moreover the velocity of this current increases in rapid proportion with the height.

It must here be observed that ascending and descending masses of air retain their local velocity of rotation, and that with increasing latitude the bed of the pole-directed current diminishes, whilst that directed to the equator increases. In consequence of the *inertia* of the flowing masses of air a steady increase of pressure in the current directed to the poles, and a diminution in that directed to the equator, will be brought about. In consequence of this combined action a general return current increasing with the cosine of the latitude of the upper polar current, will take place into the lower equatorial current. The partial change of the upper into the lower current will be brought about without any essential loss of kinetic energy by the horizontal cyclones separating the two current districts. If no rotation of the earth took place this return flow would go on to the poles without disturbance. The loss of kinetic energy by internal friction can only be small in the higher strata of the air on account of their great dimensions. These would, therefore, flow into the polar

regions from all sides with little loss of velocity, cause a block there and sink again to the earth's surface, in order to return thence as a polar current to the equator. This process would take place partly in all latitudes, and the final result would be a system of cyclones taking place in meridional planes embracing the whole atmosphere, in which the kinetic energy gained by the up flow in the lower latitudes is again destroyed by friction with the earth's surface, and by internal friction communicating the same to the higher strata of the air, or is converted into heat.

This representation of the currents is, however, essentially altered by the rotation of the earth.

In consequence of the continual transference of air from lower to higher latitudes and conversely, the air must assume a mean velocity of rotation so that the kinetic energy accumulated in the whole rotation may be maintained. As already shewn, this mean velocity of rotation corresponds to that of the 35th degree of latitude. Therefore all the air currents of the atmosphere must be displaced. Between the 35th degrees of north and south latitude both the upper and lower currents must fall behind the earth's rotation and thus be directed to the west; whilst between the 35th degrees and the poles an easterly velocity increasing quickly with the latitude, getting in advance of the earth's rotation, must prevail in both currents. The return to the equator of the upper current directed to the poles takes place therefore below the 35th degree of latitude in a westerly direction as a strengthening of the lower trade wind, and the whirling motions separating the upper from the lower currents must also assume this form of motion.

The motions of the air beyond the 35th degrees are much more complicated, whilst the upper current here wholly directed to the poles will quite keep its eastern velocity of 380 metres, as its retardation by internal friction in the higher regions of the air can only be slight, the return under current will be essentially retarded by friction with the earth's surface, and the more so the longer its course. This is also so with the meridional velocity, which in the higher layers is very slightly, but in the lower is very much diminished by friction.

If now with increasing latitude the upper current bed has become so contracted that a block takes place, the resulting local

increase of pressure produces at the same time a disturbance in the curve of the indifferent equilibrium of the atmosphere. The inflowing excess of air must, therefore, first compress the lower strata of air so that the curve of equilibrium shall be re-established down to the earth's surface. A descending current of air therefore arises, and an increase of pressure on the earth's surface dependent on the ratio of the increase of pressure in the higher regions of the air to the normal pressure belonging to it, and therefore a local maximum of atmospheric pressure. Currents of air would flow out from this region of higher pressure in a radial direction along the surface of the earth which prevent the indifferent equilibrium corresponding to the increase of pressure in the higher rarefied strata from being completely re-established. Such a maximum of pressure may, therefore, continue for a long time, and whilst it continually supplies the excess of the inflowing equatorial current to the lower return current, may even hinder the formation of a regular deflection of the upper current into the lower for a length of time. But this must finally occur, and the cause of the maximum ceases with the block in the upper layers of the air.

The return branch of the upper equatorial current must be conceived to be produced by the current hindered on its way to the pole by the block, being still more deflected by it towards the east, and so carrying with it by internal friction the deeper air layers relatively at rest or flowing in the opposite direction. It will, therefore, approach the surface of the earth in a wide curve with a slight fall, until finally uniting with the polar current it enters on its return journey to the equator. By this "carrying with it" of the deeper layers of air it will produce a rarefaction of the limiting strata of air lying under it, and thus one of the previously described opposite disturbances of the indifferent equilibrium is brought about. Thus an upflow of the deeper currents of air must restore the indifferent equilibrium, and a local minimum of pressure on the surface of the earth must result. The amount of diminution of atmospheric pressure here observed is, as with the maximum, not equal to the actual diminution of pressure brought about by the carrying force of the more quickly moving upper current of air, but to the ratio of this to the pressure corresponding to that height in the curve of indifferent equilibrium. In this

way the hitherto mysterious magnitude of the observed variations of the barometer in the middle and higher latitudes is completely explained.

The local minimum of pressure so produced will on the earth's surface draw in air from all sides, which ascends in whirls and is finally carried away with the equatorial current. It is here also the kinetic energy of the equatorial current which produces and maintains the minimum, and so sets the air in motion which streams along the ground towards the minimum. As the maximum of pressure is hence the cause of a partial return of the equatorial current, occurring in consequence of the geographical narrowing of the upper stream bed, and the path which this return current describes in the upper regions by gradual sinking marks itself by a furrow of lower pressure on the earth's surface, maxima and minima stand in a causal connection, and are, therefore, produced as a rule simultaneously and in geographical proximity. Consequently, also, the currents of air brought about by both in the lower strata of the air must combine into currents which pass essentially from maximum to minimum, but whose direction is altered by the earth's rotation in the known manner. This system of local winds must, however, finally yield to the equatorial current when it reaches the earth's surface in its gradual descent. As a rule, *i.e.*, with slight stoppage in the upper bed of the current, this will actually not take place. The return current which has commenced is completed by accumulation on the higher strata of the polar return current, and maxima and minima disappear after constant conditions of current are again brought about in the higher layers of the atmosphere. If, however, there is a considerable block, strong maxima of pressure take place, and a more rapid sinking of the equatorial return current. Over a tract of low pressure this will sink to the ground with a velocity only slightly reduced by the quick motion of relatively calm air, and will produce storms here, which begin as south-western in the northern hemisphere, following Dove's law of rotation, through west and north-west gradually becoming weaker by friction with the earth, and carrying with it relatively quiescent air, are finally absorbed in the prevailing return current to the equator. These storm winds must now produce cyclones far beyond their own limits through convection of the neighbouring layers of air, which

make it very difficult to trace the regular course of atmospheric disturbance. That the barometer will, as a rule, stand low, when the equatorial current has itself reached the ground, is explained for the most part by the convective power of the moving air rarefying the quiescent masses of air in the neighbourhood of the current. The barometer, however, shows the pressure of the surrounding quiescent air, and not the true pressure of the air comprehended in the motion. A barometer contained in the car of a balloon, moving rapidly with the storm, must, therefore, indicate an essentially higher pressure than one in a room.*

The kinetic energy active in winds and storms depends essentially according to the foregoing on the acceleration which the air rising in the tropics attains in consequence of being superheated on the earth's surface. The kinetic energy equivalent to this is transferred especially to the upper highly rarefied strata of air. On account of their inertia these are driven with slight loss of velocity by internal friction to the polar regions of the earth. They thus retain the mean velocity of rotation which they possessed when raised in the equatorial regions. They must therefore when they pass to the higher latitudes get in advance of the slower rotating earth, and so, as seen from it, approach the pole in spirals with diminishing inclination. If on this course they turn earlier to the earth on account of the narrowing of the upper current bed and, united with the returning masses of air from the higher latitudes, return to the equator, they strike these, and with a more rapid descent the surface of the earth itself with a velocity combined of their own proper velocity, and the difference between their velocity of rotation and that of the surface of the earth at the point of contact. The source from which the storms of the higher latitudes really draw their destructive energy, is therefore the inertia of the earth itself. In order that its rotation may

* Experiments I have made, of which I reserve a fuller communication, have shown that a current of air, which passes by the mouth of a thin tube placed at right angles to its direction, occasions in the tube a rarefaction proportional to the velocity of the air, which corresponds within wide limits of velocity to the pressure of a column of mercury of 0.0025^{mm} for each metre of velocity of the air. I have constructed an anemometer on this principle, which gives the velocity of the air in a very simple and not very cumbersome manner. It consists essentially of a narrow vertical tube, which is carried up as high as possible above the roof of the house. A simple pressure-measure placed in the room then gives directly the velocity of the air in metres.

remain unchanged, the law must be maintained, that the acceleration which the earth receives from the difference in velocity in high latitudes be compensated by the retardation in lower latitudes, in which the mean rotation of the air is smaller than that of the earth's surface.

It follows immediately from these considerations that with increasing geographical latitude, the frequency and strength of the currents of air in the direction of the earth's rotation, in our hemisphere the west winds must increase more rapidly. In the arctic regions themselves, the highest strata of the equatorial current, which can alone reach them without being previously forced to return, must flow down to the earth's surface in north-easterly directed spirals. They must hence, and from their being forced in on all sides to the poles, produce an arctic maximum of pressure, and after sinking down, whilst retaining their velocity, must begin their equatorial return journey as a lower north-west wind.

It is therefore again the kinetic energy gained in the equatorial upflow, which also drives back the air from the polar regions to the equator, and not the action of doubtful gradients of air pressure, which in no way suffice to explain the phenomena. By friction with the surface of the earth the south-westerly velocity which this return branch of the equatorial current possesses is soon essentially diminished, and would be altogether destroyed at the surface of the earth if the higher strata of the air of the return current did not maintain it. Owing to the expansion of the lower bed of the current which takes place rapidly in the higher latitudes a rarefaction takes place in the mean strata of the air, advancing more rapidly in the equatorial direction, which brings about an inflow of the relatively quiescent lower strata of air into the higher ones rarefied beyond the condition of indifferent equilibrium. This inflow must take place from lower latitudes because the difference of pressure producing the upflow in them is smaller on account of the expansion of the current bed. Hence the current on the surface of the earth, even in the northern hemisphere, must maintain a southerly component. This explains why, as experience proves, the south-west and not the north-east wind preponderates here, as must be the case in the higher strata of the return current.

Also, in the hypothetical case hitherto discussed of a homogeneous smooth and dry surface of the earth, the motions of the air in the middle and higher latitudes must be quite irregular, and not easy to be determined beforehand, as the maxima and minima of the air pressure commenced and maintained by stoppages and by the carrying forward of relatively quiescent air by that in quicker motion, serve as accumulators of the kinetic energy of the upper current of air, the charging and discharging of which must always cause new disturbances of the equilibrium of the atmosphere, and must produce up and down currents of air circling in it. In fact the very unequal distribution of sea and land, and the unequal hygroscopic state of the air produced by it, the orographic relations of the surface of the earth, and the diverse qualities of the ground of extensive connected tracts of it, form a chain of further disturbances in the equilibrium of the temperature, pressure, moisture, and local disturbances of strata of air accumulated or flowing near to each other which will prevent any weather prognostications from being made which may always be considered as reliable.

If moreover the hygroscopic state of the ascending air exerts no important influence on the magnitude of the kinetic energy of the air in motion, with which the energy of solar radiation is for the most part changed, it yet brings it about that the air loses its homogeneous character, since alternate strata of warm moist air and colder drier air are formed. I must refrain from entering upon the local influence of these varying conditions, for they belong to the domain of meteorology sustained by systematic observations. The same holds good of the extensive subject of local whirlwinds, as they are produced, on the one hand by local maxima and minima on the earth's surface, and on the other directly by local disturbances of the indifferent equilibrium. It only remains for me to make a few remarks on the dynamics of the latter class—the ascending cyclones with vertical axes of rotation.

I have already explained in the earlier contribution, to which I have previously referred, that the violent motions of air which occur in local cyclones cannot be well explained as the result of simple acceleration of the rising air, by an existing overheating of the lower strata of air, and by their hygroscopic condition. It appears to me altogether inadmissible to take account of the rare-

faction of the air in the interior of cyclones by the centrifugal force of the masses of air circulating round it as an accelerating force upon it. The comparative vacuum formed can only produce suction in the direction of the axis of the cyclone, either to raise the water upon the surface over which it rotates, or to draw down air from the higher regions of the atmosphere. Such a descending current of air within a cyclone is confirmed by the clear sky with a calm air often seen in its centre. One must assume that the kinetic energy of air, hastening with enormous velocity into the cyclone, and rising up into it, is accumulated in repeated accelerated impulses, and that it arises from the greater velocity of the air of the higher strata. We must then imagine a local cyclone to be produced by an upflow of superheated air brought about by some local cause, at the boundary of an upper and lower region of disturbance of the indifferent equilibrium of a quiescent atmosphere, reaching the boundary of the upper overcooled strata of air which have acquired a tendency to descend. We must then form an outer descending current around the ascending one, by which as much air is brought down as the rising current carries up. If the equilibrium current embraces extended upper and lower strata of air, the descending masses will produce an increase of pressure in the neighbourhood of the cyclone gradually extending to the surface of the earth, and on the other side to the highest regions of the air, and transfer its kinetic energy continually to new superheated masses of air, which ascend in the cyclone, whilst a portion of the descending external cyclone rotating in the same direction ascends again within the inner and transfers to it a portion of the kinetic energy gained in the upper regions. The course of the centre of the cyclone is then determined by the direction of the mean velocity of all the masses of air forming the cyclone, and its duration is that of the disturbance of the indifferent equilibrium of the atmosphere which called forth and maintains it.

Finally I will only mention that the opinion which I previously expressed, that aqueous vapour can be overcooled without condensing like water without freezing has been confirmed by recent investigations of Robert von Helmholtz. The remarkable fact that the ascent of air containing so much vapour of water over the tropical seas is not followed by ceaseless rain is thus explained. We may now assume that aqueous vapour free from dust and

particles of water reaches the higher regions of the air without being condensed.

It follows further that a local rising, like a solar protuberance, which must reach the higher regions of the air, and carry dust and particles of air, may by condensation of the aqueous vapour of these strata of air bring about the tremendous rainfalls which have been observed. The quantity of water which the equatorial current conveys to the temperate zones is thus also explained.

ON THE QUESTION OF CURRENTS OF AIR.*

IN the September number of this Journal, Mr. Möller has given, under the title "On losses of external energy in the motion of the air," an adverse criticism of my contribution "On the conservation of energy in the earth's atmosphere," of which only a very incomplete abstract is to be found in these pages. In opposition to my proposition he desires "to draw attention to the great loss of energy which the air in motion suffers." That is to say that the loss of kinetic energy which would be caused by the friction of the air on the rough surface of the ground is of much more importance as regards "existing circulations" than the influence of the conservation of the kinetic energy and its consequences. He seeks shortly to support this opinion "by contrasting the properties of two depressions existing over a continent, of which the one possesses a preponderating ascending and the other a preponderating descending current." It is incomprehensible to me what the explained differences of the directions of the current brought about by these different local depressions have to do with the conservation of energy. The author indeed leaves the main question quite untouched:—whence there arise these considerable stores of energy which represent maxima and minima of pressure? According to the view which I support, the great reservoir from which the energy is derived, that produces maxima and minima pressures of the atmosphere, and is able at times to set in violent

* *Meteorologische Zeitschrift*, December, 1887.

motion the strata of air lying motionless near to the surface of the earth, is to be sought in the great and continuous circulation of the atmosphere occasioned and maintained by the superheating of the equatorial latitudes by the sun's rays, and which continuously conveys a great portion of the equatorial heat to the temperate and cold zones. That the friction of the air with the surface of the earth along the course of this great current, comprising the whole atmosphere, should exert so important an influence (greater than the conservation of the energy) must be contested. As is well known there is, strictly speaking, hardly any friction between gases and solid bodies, as the latter are enclosed in a closely adhering air layer which does not take part in the motion of the air. And as, according to Bessel's experiments, a layer of air adhering to the surface of a pendulum swings with it, a layer of air also cleaves to the surface of the earth, which in close proximity to the ground takes no part in the currents of air of the atmosphere, and at a greater distance hardly any. There hence results a relatively quiescent, as it were stagnant, layer of air on the surface of the earth, whose height depends on its roughness, and is consequently less high above the sea and great plains than above mountainous and woody lands. This relatively quiescent layer of air rotating uniformly with the surface of the earth is in a great measure subject to disturbances owing to locally ascending currents and to cyclones brought about by them, and it may be correct as regards it that the effect of friction upon the circulation taking place is more effective than the *inertia*, and therefore than the conservation of the kinetic energy. It is, however, only proportionately small forces which make their appearance in such local disturbances of equilibrium and cyclones. It is only when the cyclones existing below reach up to the region of the higher continuous atmospheric current, and draw them into its motion, or when the twistings of the current and cyclones, called forth in the region of the upper general current through perpetual narrowing of the bed of the current of the equatorial, and widening of that of the polar current, bring about local changes of pressure, and in consequence the quick flowing air of the upper regions flows back to the earth's surface, its inertia makes its appearance there as furious motion of the air. Mr. Möller will not, so it appears, allow this reaction of the general current of air principally

existing in the higher air layers which are not hindered by friction on the earth's surface, on the phenomena observed on the earth's surface, but I must acknowledge that I do not understand the mechanical basis on which he will explain it. Depressions and maxima pressures of the air cannot occur of themselves. As much kinetic energy has to be used in their formation as they can reproduce under the most favourable conditions. Only the sun's radiation can produce stores of force and currents of air, which fall most of all in the torrid zone; whence it already follows that we have to seek the causes of the changes of pressure and storms of the middle and high latitudes in the equatorial up-flow and not in local phenomena.

But Mr. Möller does not restrict himself to seeking to prove that the conservation of energy in the atmosphere is not necessary for an explanation of the phenomena, but seeks in a special chapter entitled "Mixture of masses of air" to show that the assumption lying at the basis of my hypothesis on the circulation of air, viz., that the mixture of masses of air of different velocity takes place according to the law of the conservation of kinetic energy, is false. "The resulting external motion takes place according to the law of inelastic, and not to that of elastic impact. Hence masses of air of different velocity must have after mixture the mean velocity of the masses and not the same kinetic energy. The loss of the latter must be changed into heat, sound or electricity."

I am, fortunately, not here compelled to enter upon the obscure chapter of inelastic impact, which is incapable of explanation by experiment, as there are no inelastic solid bodies, because a mixture of air of different velocity does not enter into my argument. In my view it is the same unmixed air, which rising up accelerated at the equator, without changing its velocity of rotation, streams into the poles in the highest regions of the atmosphere, and which, according to the narrowing of its current bed with increase of latitude, returns in the lower steadily widening current bed to the equator, retaining essentially its original velocity of rotation. I only brought forward the idea of a mixture in order to calculate in a simple manner the amount of the constant velocity of rotation. It cannot be doubted that it actually exists, if one allows the interchange of air in meridional currents,

as regularly manifested in the trade winds. The westerly direction of the trade winds shows indeed that the friction of the body of the earth does not itself suffice to communicate to the lowest strata of the air the local velocity of the rotation of the earth, and it must therefore be perfectly clear that the higher masses of air, which are almost entirely withdrawn from the effect of friction with the earth's surface, retain unaltered the mean velocity of rotation with which they ascend. Indeed there are no forces to bring about any alteration in it. My calculation of this velocity of rotation of the atmosphere is based upon the assumption that the meridional forward and backward current is extended equally over the whole surface of the earth. In this case the idea I used of a mixture gives the correct mean velocity. It is evident that this assumption cannot be strictly correct, as the angular velocity of the air with constant velocity of the periphery must be so great in the neighbourhood of the pole that the air pressure would be indefinitely small. The reversing of the polar air current must therefore be already accomplished before the polar regions are reached, which must themselves be covered with relatively quiescent or whirling air. The mean velocity of rotation of the atmosphere must therefore lie somewhat nearer to the equator than the 35th degree of latitude calculated by me.

It has been pointed out by Mr. Möller and also previously by others, that Ferrel had already shown by calculation that the atmosphere above the 35th degree of latitude had the velocity of rotation of the earth's surface. This is quite correct, but is according to all appearance only an accidental coincidence, for the bases of the two calculations are different throughout, and did not, as I myself at first assumed, proceed from similar considerations. With Ferrel the westerly motion of the air between the 35th degrees of latitude and the easterly motion polewards of those degrees result from a calculation which takes no notice of inertia and friction, as if it were an original condition of motion, which had to alter with the influence of modifying forces, whilst my calculation has regard to the final condition of equilibrium.

I assume with Dove that a powerful equatorial current is the cause and the conveyer of the alterations of the pressure of the air, as of the cyclones and storms of the higher latitudes. When Dove started his theory and advocated the same with a warmth

and faith of conviction well remembered by many older physicists, treatises on the theory of the conservation of energy did not exist. Although the theory was only imperfectly developed and entailed many impossibilities, Dove discovered in the "ascending current" of the torrid zone the source which as equatorial current conveys heat, power and vapour of water to the upper latitudes. It may appear very wonderful to the younger generation of physicists, brought up in the light of the theory of the conservation of energy, that a Dove did not think of freeing his theory of the equatorial current from its dross by strict consideration of the vis inertia of moved masses. He need only have supposed his "ascending current" which rose as a north-east trade, and was deflected by the south-east trade obstructing the way at the equator in a north-westerly direction, to continue its course with unaltered velocity and direction, then the smaller velocity of rotation of the equatorial current which manifested itself as an east wind on reaching the higher latitudes with less velocity of rotation, must become at first equal to, and with farther advance greater than the velocity of rotation of the earth, and so the constant east wind of the tropics must change to the preponderating west wind of temperate and polar zones. Also the difficult question according to Dove's theory regarding the cause of the return current to the equator with considerable increase of pressure of the upper latitudes may be explained by the inertia of the moved air. As with ascending latitudes the current bed of the equatorial current continually narrows, the bed of the polar current widens in the same degree; with the first there must therefore occur a continuous retardation with increase of pressure, with the last a spreading out with reduction of pressure. The consequence must be a continuous overflow of air from the equatorial to the polar current, whereby the difference of pressure is partly again neutralized. This overflow can produce no alteration in the velocity of rotation of the equatorial current, and so the polar stream must in general have the same velocity of rotation as the former. The meridional current will also remain unaltered through passing over from the equatorial to the polar current, for this passing over is performed as a rule in curves with great radius. It is therefore the inertia of the air of the equatorial current which drives it again in the polar current to the equator.

With this alteration in the direction of motion must necessarily be combined an increase of pressure towards the poles, but this is in part compensated by the inverse action of the angular velocity increasing with the latitude, and the inclination of the plane of rotation to the direction of gravity.

It is evident that this dependence on one another of all moving factors, makes the circulation of the air very sensible to disturbances which are, as a rule, to be reduced to local disturbances of equilibrium in the lowest relatively quiescent layer of air. When such disturbances of equilibrium develope into great tornadoes in the upper air strata—which is much favoured by the opposed direction of the currents of air lying over one another—they must call forth in it various alterations in the pressure of the air. These alterations of the air disturb on their side the indifferent equilibrium existing in the greater heights of the atmosphere, and thus occasion increased alterations in the pressure of the air on the earth's surface, and therefore produce maxima and minima of air pressure with all their consequences.

ON THE GENERAL SYSTEM OF WINDS ON THE EARTH.*

IN the May number of the "*Meteorologische Zeitschrift*" there is a paper by Dr. Sprung, entitled "On the Theories of the General System of Winds on the Earth," in which the calculations of the direction and strength of general currents of air set forth in my communication to the Academy of the 4th March, 1886, "On the Conservation of Energy in the Earth's Atmosphere," are critically compared with Ferrel's old theory. This paper induces me to make the following observations, which, however, are not directed against Dr. Sprung's objections to the strict validity of the results of my calculations, which objections are to a certain extent quite

* From the transactions of the Berlin Academy of Science, 1890, Vol. XXX.

just, but against the supposition that I have attempted, like Ferrel, "to found on theoretical calculations a theory of the general system of winds on the earth." Apart from not considering myself a sufficient adept in mathematics, I may say that I consider this method altogether inappropriate. So very complicated a problem as that of the general system of winds cannot possibly be constructed *à priori* on the basis of mathematical calculations, as up to now no simple basis has been found underlying all the phenomena. In my treatment "Of the Conservation of Energy in the Earth's Atmosphere," I first sought to determine the forces which produce, maintain, and retard the motion of the air, and then to find by calculation the direction and magnitude of the general motion of the air induced by their combination. It is therefore not correct "that I sought like Ferrel to demonstrate by means of calculation an original state of atmospheric motion in order afterwards to base my further speculations thereon." Nor is it correct that I have taken no account in my calculations of the retardation of the motion of the air through friction, for the meridional flow of air, very appropriately called "main circulation" by Dr. Sprung, on which my theory of the general system of air-currents is based, depends exactly on the balance between the upward acceleration of the air in the equatorial regions caused by the heating of the lowest strata through solar radiation, and the loss of energy which the air in motion suffers during its circulation. This main circulation has in the course of thousands of years brought about an admixture of masses of air, which without it would have rotated with the surface of the earth on which they rest. I only made use of the mathematical idea of an instantaneous frictionless admixture of the strata of air of all latitudes, to determine in a simple manner the direction and magnitude of the motion which has existed since remote antiquity. Ferrel does not start, as I do, from a main circulation which continually interchanges the strata of air rotating with the velocity due to their latitude, and thereby gradually intermixes them, but brings about this admixture through a frictionless displacement in the meridional direction of the rotating rings of air of the different latitudes in a manner not very clearly explained. We have here essentially the same basis of calculation as that underlying my idea of an admix-

ture, and Ferrel's and my calculation are therefore the same in result as regards the direction of the currents of air ; on the other hand, we differ essentially in our statements of the relative strength of the wind north and south of the 35th degree of latitude. I am quite of Dr. Sprung's opinion that neither of the two theories can be considered quite correct. I look upon mine indeed as nothing more than a first approximation to the truth. And so in my calculations I have not taken into consideration such complicating influences as the diminution of temperature towards the poles, and the non-coincidence in direction of centrifugal force and gravity. The latter fact, the effect of which is also proved by the consideration that the mass of air rotating in the higher latitudes must everywhere have a tendency to continue moving in the greatest circles, and therefore to strive to reach the equator, would bring about a diminution in the pressure of the air with approach to the poles, and would therefore essentially prejudice the result of the calculation of the admixture, if this tendency were not compensated by other forces having an opposite effect. It is not, however, these but other fundamental assumptions which determine a very great difference between the two conceptions and lead to very different results. One of them is the assumption by Ferrel that the so-called law of areas in the form of the conservation of the moment of inertia holds good for the displacement of the air rotating with the earth's surface in a meridional direction. To this I cannot agree, but, on the contrary, must deny that the conservation of the moment of inertia comes into play in the motion of the air.

This law of areas, taken from astronomy, states that a mass which rotates freely about another describes equal areas in equal times. This is due to the acceleration of the rotating mass as it approaches and its retardation as it recedes from the centre of attraction of the fixed mass. The greater velocity acquired by acceleration results in the description of a larger arc in the unit of time, and so leads to the law of areas. According to Ferrel a quantity of air rotating in any latitude with the earth's surface cannot travel in the direction of the meridian with an invariable absolute velocity, and therefore with a constant *vis viva* as I assume it to do, but its moment of inertia must remain constant, which corresponds to a

considerable change of velocity. In order that the moment of inertia may remain constant (which is the case when the linear velocity of the rotating body varies so that equal areas are described by it in equal times), a considerable amount of energy must be expended to produce the alteration in the velocity of the inert mass of air. But there is no force whatever available to perform this work. If the radius of rotation of a rotating solid body is shortened, the force which produces the shortening must overcome the centrifugal force. The sum of the products of all the centrifugal forces into the paths traversed gives the work expended in the acceleration of the rotating mass, and this exactly suffices to maintain the law of areas, that is, in this case the moment of inertia constant. But no analogous relations exist in the case of the motion of the air on the earth's surface, where no alteration in the force of attraction is caused by the tangential displacement and no acceleration of the shifting mass by gravitation. Nor is it clear how the neighbouring air-strata can exert a pressure on those to be displaced capable of performing the considerable work of acceleration which the maintenance of the moment of inertia requires. A displacement of the whole mass of air of a rotating ring in the meridional direction cannot moreover take place, for the volume of such a ring of definite thickness varies as the cosine of the latitude, and with a polar displacement a corresponding portion of the mass of the ring must either lag behind or return to the equator. But even as regards that portion of the ring actually displaced in the polar direction, there is no physical reason for assuming a conservation of the moment of inertia in the case of currents of air; such an assumption would, on the contrary, lead to the greatest contradictions and discontinuities. For in the assumed original condition from which both Ferrel and I start, when as yet no meridional currents existed, the air of each latitude rotated with the velocity of the ground upon which it rested, and its velocity therefore diminished with the cosine of the latitude. Now, according to Ferrel's views, this relation must not only have been reversed with the appearance of meridional currents, but instead of a diminution an increase of the velocity of the air must have occurred in a yet higher degree, if the moment of inertia of the air is supposed to remain constant. But why it must remain constant, and what forces could bring about so considerable an

increase of the *vis viva* of the rotating air, are equally incomprehensible.*

Another of Ferrel's assumptions with which I cannot agree is that the uppermost strata of air can slide down inclined surfaces of equal air-pressure, for in these there is as little tendency to tangential displacement as on a level surface. That such a displacement could not in general take place follows at once from the consideration, that even if a current of air did descend, a difference of pressure would arise, with a consequent disturbance of the balance of pressure, and the immediate production of a back current. It hence follows that meridional currents of air cannot be caused by the steady increase in the temperature of the air from the poles to the equator which (disturbances excepted) is found to exist, and to this Dove agrees. Surfaces of equal pressure exist at all levels throughout such an unequally heated atmosphere which reach from the equator to the poles, and along which no spontaneous motion of the air can take place. Notwithstanding the greater rarefaction or loosening of the air through the heat of the torrid zone, the air would remain at rest, if the indifferent equilibrium were not disturbed in some portion of it. The real condition of equilibrium and of relative rest of the atmosphere is that of indifferent equilibrium with the adiabatic scale of temperature appropriate to it. In other words, setting aside friction, no work is required to take a mass of air from one level to another, which means in this case that the energy expended by the expansive action of the air is balanced by the loss of heat in cooling, and inversely. The condition of relative rest of the atmosphere depends therefore on the maintenance of indifferent equilibrium, every disturbance of which makes its

* I must emphatically protest against Dr. Sprung's statement, "that there is the same (certainly very excusable) error contained in my assumption of a constant velocity of rotation of the air, as in the conception of Hadley and Dove on the influence of the earth's rotation on the motion of the air." Dr. Sprung is not entitled to bring forward von Helmholtz's communication "On Atmospheric Motions" in support of this opinion, for in this mathematical inquiry von Helmholtz has considered a "hypothetical case." He says, "If we assume a rotating ring of air—the axis of which coincides with the earth's axis—to be displaced at one time to the north and at another to the south by the pressure of similar neighbouring rings, the moment of inertia must remain constant, in accordance with the well-known general mechanical principle." This is no doubt correct, for in the assumed case the pressure of the neighbouring rings performs the work of acceleration; but the point in question is just whether there are forces available to exert this displacing pressure.

appearance as a storage of energy, having the tendency to re-establish it through the motion of the air. These disturbances are to be explained exclusively by the unequal heating of the strata of air by the sun's rays and their unequal cooling by radiation of heat into space. The sun's rays heat the surface of the earth, which then heats the strata of air resting upon it. The excess of temperature thus brought about over the adiabatic temperature of the earth, which corresponds to the mean temperature of the whole superincumbent column of air, forms a store of energy like a bent spring, which can only be balanced again through the spreading of the excess temperature of the deepest to the overlying air-layers, and thus restoring the disturbed indifferent equilibrium. Practically, this can only be effected by air-currents. When the superheating is only local, a rising of the superheated air will occur at some locally favoured place, which will increase quickly with the height, for the upflow in the so-formed natural chimney increases with the height. This chimney differs from ordinary chimneys not only in height, but essentially, in so far that it has elastic walls, and that the pressure and density of the strata of air inside and outside diminish with the height. The velocity of the ascending air must therefore increase inversely as the density, for at each instant equal masses of air must pass through all sections of the chimney. Considering the small height of the atmosphere as compared with the earth's radius, no considerable increase of area with height takes place within it, and hence the velocity of the currents of air with up and down flow must increase and diminish absolutely with the air-pressure of the place. Through this upflow a greater portion of the sun's energy stored up in the air is therefore changed into the *vis viva* of air in motion than would be the case without such acceleration. The final results of the upflow of air limited as to space which has been overheated on the earth's surface, will be that this local air-current ascends with accelerated velocity up to the very highest regions of the air, that at the same time the strata of air surrounding the upflow descend with diminishing velocity, and that finally the heat produced on the earth's surface, and thereby disturbing the equilibrium, is dispersed through all the overlying air-strata and so restores the indifferent equilibrium of this part of the atmosphere.

When this heating of the layers of air near to the ground is

extended to a whole zone of the earth, the balancing of the indifferent equilibrium disturbed through solar radiation is effected on the same principle, although the resulting phenomena are quite different. In this case the upflow would no longer be limited as to space, but comprehend the whole torrid zone systematically, nor can it be limited as to time, but the adjustment, like the cause of the disturbance, must continue unlimited. A continuous system of currents comprehending the whole atmosphere must consequently be formed, which eventually brings about the continual transference of the extra heat of the air near to the ground in the torrid zone to the whole atmosphere at all levels and latitudes, and restores the indifferent equilibrium disturbed in the torrid zone, through continuous air-currents.

If the possible lines of currents are constructed, bearing in mind that stream-lines cannot cut one another, that the velocity of an ascending current of air must increase directly as the height and inversely as the pressure, and finally that the air must maintain its velocity unaltered unless expended in friction, mixture, or the work of compression, the system of winds proposed by me is necessarily arrived at, depending essentially on the *vis inertia* of the heated air set in accelerated motion by the equatorial upflow. This *vis inertia* not only forces the accelerated rising air along the higher strata of the atmosphere, but is also the cause of its return along the lower strata to the equator.

It would lead me beyond the scope of this communication were I to enter upon a closer discussion of this *vis inertia* of the air, and on the influence of the vapour of water, which considerably modifies it. But I may be allowed to say something on the origin of the great stores of local energy indicated by the maximum and minimum pressure of the air. The sum of the pressure of the atmosphere at every part of the earth's surface must be constant, for this sum represents the weight of the invariable total amount of air. A reduction of the pressure of the air at one part must therefore necessarily be always opposed by a simultaneous increase of pressure elsewhere. It is evidently no use to look for the originating cause of the maxima and minima in local conditions of the atmosphere. The barometer frequently gives intimation of these before any alteration takes place in the state of the atmosphere on the earth's surface. Frequently, however, light clouds

indicate an alteration already set in the higher strata ; and so in my paper, "On the Conservation of Energy in the Earth's Atmosphere," I placed the origin of the maxima and minima in the upper strata of the atmosphere, in the temperature and velocity of which constant alterations take place, depending upon the place of ascent of the air, *i.e.*, on its temperature and hygroscopic state before it ascends. If no change in the seasons took place, there would probably be great regularity in the currents in the upper strata of the atmosphere, which would bring about a certain sequence in the changes of weather not hitherto attained. We are not yet able to find out where the air comes from which flows at any moment over any portion of the earth's surface in a polar direction along the higher strata of the atmosphere, and upon the place and the season of the ascent will depend the temperature and velocity of this air. As the expenditure of heat during the ascent of the air, *i.e.*, during its working expansion, depends upon the degree of its attenuation, and therefore on its elevation, nearly the same diminution of heat takes place with hot as with cold air. At all heights of the atmosphere differences of temperature must make their appearance equal in degree to those existing on the earth's surface, for any surplus of heat which the air possesses before its ascent must be retained by it after it has been rarefied and cooled through the ascent. On this account the general condition of the atmosphere will not be that of unstable equilibrium but so-called stable equilibrium, for the higher strata of the atmosphere, on account of their equatorial origin, will be warmer and lighter throughout than they should be according to the adiabatic scale of temperature of the place over which they are. The velocity of ascent of the air will increase with the quantity of heat and moisture which it contains before its ascent ; and therefore in the higher strata of the atmosphere of the mean and higher latitudes, relatively warm and light currents of air of greater velocity must intermingle with colder and slower currents. Such a current of relatively light and warm air, completely or partially occupying the upper atmosphere, disturbs on its side the indifferent equilibrium of the lower strata of the atmosphere. The lower atmosphere, relatively at rest on the boundaries of the strata, must be under too great a pressure, and must therefore expand and be carried along with the quick-flowing lighter air

above it. As von Helmholtz has shown, this carrying away must go on with great energy, producing waves, and in consequence there must be an expansion and flowing up of the lower air, which will continue until the indifferent equilibrium disturbed by the lower pressure of the upper strata of the atmosphere is again restored. When the pressure of the air of the upper strata increases beyond that due to their elevation, through cooling and condensation, in consequence of the diminution of the width of the current with increasing latitude, the opposite effect will be produced, the limiting strata will sink down, condensing the lower strata of air, and thereby increasing their pressure. In both cases the disturbed indifferent equilibrium must eventually be again restored by the strata of air lying below the source of disturbance delivering by means of up-currents, or taking up by means of down-currents, as much air as is required to restore the condition of indifferent equilibrium in the whole height of the atmosphere. In order to effect this the pressure of the air of the lower strata must increase or diminish, in the first instance, until it has adjusted itself to the scale of pressure of the indifferent equilibrium of the disturbing upper air-strata. In other words, the pressure on the earth's surface must vary proportionately with the change of pressure above, by which means the sudden changes observed in the pressure of the air on the earth's surface are perfectly explained. This alteration in the condition of the lower strata of air will continue even after this adjustment has been effected so long as the cause in the upper strata of the atmosphere continues; and during that time minimum pressures of air with ascending currents, and maximum pressures with descending currents, of air must occur, and set the atmosphere for a considerable distance in whirling motion. It is only when the flow of air in the higher strata of the atmosphere has again become normal, that a mean position of the barometer and relative rest of the atmosphere on the earth's surface will again prevail.

The theory of the general system of winds may therefore be summed up in the following statements:—

1. All motions of the air depend upon disturbances of the indifferent equilibrium of the atmosphere, and tend to bring about its restoration.

2. These disturbances are caused by the superheating of the

strata of air lying nearest to the earth's surface through solar heat, by unsymmetrical cooling of the upper layers of the air through radiation, and by the piling up of masses of air in motion through obstructions occurring to the current.

3. The disturbances are balanced by means of ascending currents, which possess an acceleration of such a kind that the increase of velocity of the air is proportional to the diminution of its pressure.

4. Down-currents of equal magnitude correspond to the up-currents, and in these the velocity of the air is retarded in the same proportion as that of the upflow is accelerated.

5. If the heating of the lower strata of air takes place within a limited area, a local upflow occurs reaching to the uppermost regions of the air, and presenting the appearance of whirling columns with ascending spiral currents of air inside and similarly directed descending currents outside. The result of these whirling currents is a diffusion of the surplus heat of the lower strata through which the adiabatic equilibrium is disturbed, to the whole column of air which took part in the whirling motion.

6. When the sphere of disturbance of the indifferent (or adiabatic) equilibrium is very extended, comprising for instance the whole torrid zone, the equalization of temperature can no longer be effected by local ascending whirling currents, but these must comprise the whole atmosphere. The conditions are the same as with local currents, viz., an accelerated ascent and retarded descent of the air, so that the velocity of the air due to the action of the heat is at the different latitudes approximately inversely proportional to the air-pressure prevailing there.

7. As the air of every latitude rotates with approximately the same absolute velocity in consequence of the constant meridional currents which the heat produces and maintains, the great system of currents of air surrounding the whole earth, whose function it is to give a share of the surplus heat of the torrid zone to the whole atmosphere by transferring equatorial heat and moisture to the middle and higher latitudes and by originating local air-currents in them.

8. These latter are due to the local production of alternate increase and decrease of pressure through the disturbance of the indifferent equilibrium in the upper strata of the atmosphere.

9. The maximum and minimum air-pressures are effects of the temperature and velocity of currents of air in the higher strata of the atmosphere.

From what precedes, the investigation of the causes and effects of the disturbance of the indifferent equilibrium of the atmosphere may be considered as one of the most essential problems of meteorology, and the investigation of the geographical origin of the air-currents passing over us on their way to the poles as the most important problem in the prognostication of the weather.

ON THE QUESTION OF CAUSES OF ATMOSPHERIC CURRENTS.*

It was not my intention to return to the question of atmospheric dynamics, because I believed I had made my view sufficiently clear, and as Mr. Max Möller, who alone in his treatise "The use of the law of surfaces on atmospheric currents" has gone closer into my communication "On the general system of winds of the earth," has consented in its essentials to my assertion of the inapplicability of the so-called law of surfaces to atmospheric currents. But I have lately become aware that certain statements of principle of Mr. Möller are directly opposed to those put forward by me, and that my silence would be construed as though I had given them up. As this is by no means the case, and as it does not appear to me in the interest of science to let contradictions of principles remain unanswered, I must beg leave for a few lines in vindication in the columns of this paper.

Mr. Max Möller writes, "The author directs against Ferrel's theory the consideration, that it makes a much too unlimited use of the law of the conservation of the moment of inertia. This is correct as a matter of fact, although a limitation of the succeeding words of the author appears necessary. He says, 'I must deny that the conservation of the moment of inertia comes into play in the motion of the air,' and further, 'that the moment of inertia

* *Meteorologische Zeitschrift*, 1891.

may remain constant, a considerable amount of energy must be expended. But there is no force whatever available to perform this work. In the sentence quoted the word 'whatever' should be left out. We shall see that actually no inconsiderable meridional working forces are at our disposal, that they however certainly do not suffice in the end to maintain a simple closed circular motion of the air between the equator and the pole."

Mr. Max Möller then proceeds to look for the existing forces for the conservation of the moment of inertia. He says, "The greatest force of the atmosphere which is available for the conservation of the moment of inertia is the meridional component of gravity which follows from the flattening of the earth. In moving a material point from the equator to the pole on the surface of the earth, the material point sinks in the direction of the attractive force of the earth 11,000 metres. The work given out amounts for a mass of 1 kilogramme to 11,000 kilogrammetres."

Now this would be quite right, if the mass of one kilogramme which is shifted from the equator to the pole did not take part in the rotation of the earth, as Mr. Möller in another place puts forward. But as it takes part in the rotation of the earth, it has to overcome in its meridional passage to the pole the centrifugal force which tends to bring it back to the equator. The retardation produced in this way must be everywhere exactly equal to the acceleration due to the component of gravity directed to the pole in consequence of the flattening of the earth, for this equality is indeed the condition of the earth's ellipsoidal form. There can therefore be no question of an accelerated sinking down of a point or body on the sloping surface of the earth flattened in consequence of rotation. This is also the case with every other point of the mass of the earth assumed to be freely movable. Equilibrium must always exist between the component of the force of attraction directed to the pole, and the component of the centrifugal force directed to the equator, as otherwise motion would take place and the form of the ellipsoid would be altered. This may be otherwise stated: that an infinite number of level layers may be taken through the mass of the earth, supposed to be freely movable, in which no tendency to any forward motion exists. It hence directly follows that also in the atmosphere itself only dynamical

but no statical tendency to motion can be operative, that therefore *all atmospheric motion can only be the work of the sun*. When this fundamental law has been recognised, then the sea of air rotating with the earth's surface cannot be subject to any accelerations altering the sum of its kinetic energy acquired through the earth's rotation and solar work. Neither in the upper nor lower atmosphere can a flow in the direction of the poles exist which works acceleratingly upon it. As at every moment exactly as much air must flow to the poles as to the equator through each parallel of latitude, to maintain the equilibrium of pressure it cannot be conceived by what forces, the air which has glided down the assumed oblique plane can be again driven up. There is no other force available for this return motion of the air to the equator than the kinetic energy accumulated in it. Although I fully concur with Mr. Möller, that the velocity communicated to the air by the sun's heat driving it up is small compared with that communicated to it by the rotation of the earth in which it takes part, it is yet to be remembered, that the meridional energy of motion communicated to the air by the sun's heat in a cycle is lost only to a slight extent by friction. The trade winds teach this, which restore to the equatorial ascending current the major part of the meridional energy of motion previously received. This is accumulated in the course of centuries, and it alone sets and maintains the atmosphere in motion, and produces maxima and minima of the air pressure through the whirls which are caused by the reversal of the polar into the meridional direction, brought about by the continued narrowing of the upper and simultaneous widening of the lower bed of the current.

THE END.

**RETURN TO: CIRCULATION DEPARTMENT
198 Main Stacks**

LOAN PERIOD	1	2	3
Home Use			
	4	5	6

ALL BOOKS MAY BE RECALLED AFTER 7 DAYS.

Renewals and Recharges may be made 4 days prior to the due date.
Books may be renewed by calling 642-3405.

DUE AS STAMPED BELOW.

MAY 10 2003		

FORM NO. DD6
50M 5-02

UNIVERSITY OF CALIFORNIA, BERKELEY
Berkeley, California 94720-6000

U. C. BERKELEY LIBRARIES



065442638

